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HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.

JULY—DECEMBER.

1866.



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THE
HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES:

BEING

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HALF-YEARLY ABSTRACT

OF THE MEDICAL SCIENCES,

ETC.

PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE.

(A) CONCERNING HYGIÈNE.

ART. 1.—*On the Mode of Propagation of Cholera.*

By Mr. JOHN SIMON, F.R.S., Medical Officer of the Privy Council.

(*Official Memorandum, July, 1866.*)

THE following observations on the mode of propagation of cholera are from the official memorandum of the Medical Officer of the Privy Council, on the precautions to be taken against the epidemic under the Regulations issued by the Lords of the Council and otherwise:—

“That such precautions (never unimportant where human health is to be preserved) are supremely important when the spread of cholera is to be prevented, is a truth which will best be understood when the manner in which cholera spreads is considered. Happily for mankind, cholera is so little contagious, in the sense in which small-pox and typhus are commonly called contagious, that, if proper precautions are taken where it is present, there is scarcely any risk that the disease will spread to persons who nurse and otherwise closely attend upon the sick. But cholera has a certain peculiar contagiousness of its own, now to be explained; which, where sanitary circumstances are bad, can operate with terrible force, and at considerable distances from the sick. It appears to be characteristic of cholera—not only of the disease in its developed and alarming form, but equally of the slightest diarrhœa which the epidemic influence can produce—that *all matters which the patient discharges from his stomach and bowels are infective*; that the patient’s power of infecting other persons is represented almost or quite exclusively by those discharges; that they, however, are comparatively non-infective at the moment when they are discharged, but afterwards, while undergoing decomposition, acquire their maximum of infective power; that, if they be cast away without previous disinfection, they impart their own infective quality to the excremental matters with which

they mingle, in filth-sodden earth or in depositories and conduits of filth, and to the effluvia which those excremental matters evolve; that, if the infective material, by leakage or soakage from drains or cesspools, or otherwise, gets access, even in the smallest quantity, directly or through porous soil, to wells or other sources of drinking-water, it can infect, in the most dangerous manner, very large volumes of the water; that the infective influence of choleraic discharges attaches to whatever bedding, clothing, towels, and like things have been imbued with them, and renders these things, if not disinfected, capable (as the cholera patient himself would be capable, under the same conditions) of spreading the disease in places whither they are sent for washing or other purposes; that, in the above-described ways, even a single case of disease, perhaps of the slightest degree, and perhaps quite unsuspected in its neighbourhood, may, if local circumstances co-operate, exert a terribly infective power on considerable masses of population. 'If local circumstances co-operate,' however, is the stated condition for that possibility; and it will be observed that the essence of the sanitary precautions, which have been recommended to nuisance authorities and others, is to annihilate those 'local circumstances.' The choleraic infection does not seem able largely to injure any population unless a filthy state of things be pre-supposed. It is pre-supposed that the atmosphere or the drinking-water of the population is impure with the most loathsome of impurities—that the infective material has had opportunities of action which decent cleanliness would not have afforded it—that, in inefficient drains or cesspools, or other like depositories, it has had time to develop its own infective power, and to render other stagnating filth equally infective with itself; and that, from such foci of infection, the disgusting leaven of the disease has spread, in air or water, to be breathed or swallowed by the population. In this view of the case, it will be understood that works of sewerage, house-drainage, and water-supply, properly executed and properly used, give to town populations an almost absolute security that cholera, if introduced among them, can have no means of spreading its infection. And equally it will be understood that, in the absence of those permanent safeguards, no approach to such security can be got without incessant cleansings and disinfections, or without extreme vigilance against every possible contamination of drinking-water."*

* "If unfortunately the only water which for a time can be got should be open to suspicion of dangerous organic impurity, it ought at least to be boiled before it is used for drinking, but then not to be drunk later than twenty-four hours after it has been boiled. Or, under medical or other skilled direction, water, in quantities sufficient for one day's drinking in the house, may be disinfected by a very careful use of Condry's red disinfectant fluid. This should be added to the water (with stirring or shaking) in such number of drops that the water, an hour afterwards, shall have the faintest pink colour which the eye can distinctly perceive. Filtering of the ordinary kind cannot by itself be trusted to purify water, but is a good addition to either of the above processes. It cannot be too distinctly understood, that dangerous qualities of water are not obviated by the addition of wine or spirits."

ART. 2.—*On Disinfection, with especial reference to Cholera.*

By Dr. MAX PETTENKOFER.

(Schmidt's Jahrbücher, 1866, Band cxxxi.)

The following is an abstract of a paper published in the *Zeitschrift f. Biologie*, 1866:—

The excretions of cholera patients are, in their recent state, generally either neutral or feebly alkaline; but after the lapse of a short time they become decidedly alkaline. All means which prevent the excretions from becoming alkaline change the ordinary course of their decomposition, and all disinfectants against cholera are to be valued accordingly as they are able to prevent the commencement of ammoniacal decomposition in the urine and fæces. The agents that fulfil this purpose are, metallic salts, mineral acids, and carbolic acid, by means of which urine and fæces may be preserved for many months in an acid condition. Among metallic salts, the sulphate of iron deserves the preference, both on account of its activity for the object to be attained, and also on account of its cheapness and of its ready accessibility in any quantity. The quantity required will depend upon whether the excretions to be disinfected are still recent, or have already undergone ammoniacal change. For the disinfection of cesspools, where change has already taken place, the salt must be added in concentrated solution until the odour of ammonia and of sulphuretted hydrogen is completely removed, or until the contents of the pit, after stirring, have an acid reaction. For recent excrement, the average quantity required for each person will be about an ounce daily. The most certain method is to have the excrement disinfected before it is allowed to pass away into the pit or sewer. It does not lose its proclivity to every kind of change, nor all fætor; but the ammoniacal change will with certainty be delayed for months by the addition of the above-mentioned quantity of sulphate of iron. The excrement may therefore always be conveyed away from the vicinity of human dwellings while still in an acid state. The fætor cannot be removed by any means, and can only be concealed by stronger penetrating odours.

The prevention of ammoniacal change in excrement may also be effected by mineral acids, as the sulphuric and hydrochloric, and also by carbolic acid; but the practical applicability of these agents is very limited when compared with that of sulphate of iron. If excrement already ammoniacal be treated with sulphuric or hydrochloric acid, much carbonic acid and sulphuretted hydrogen will be developed—substances the development of which should be avoided with all possible care. The mineral acids, moreover, act not only upon iron and zinc, but energetically upon the mortar of the pit, and would be neutralized by it. Sulphate of iron, on the contrary, prevents the formation of sulphuretted hydrogen, and affects neither iron, zinc, nor mortar, nor is neutralized by them.

Another agent that possesses some, if only a limited, applicability as a disinfectant is sulphurous acid. This is certainly effectual in preventing the development of sulphuretted hydrogen, which it decom-

poses to form sulphur and water. Since, however, it is not alone in the destruction of sulphuretted hydrogen, but far more in the establishment of an acid reaction, that we seek the essentials of disinfection, the use of sulphurous acid alone is attended by the same disadvantages as the use of the other mineral acids. In the combined form, as a sulphite, the sulphurous acid is wholly inoperative, and cannot in the least delay the commencement of alkaline reaction. It even seems to conduce to this reaction; for urine treated with sulphite of soda containing some excess of acid, then neutralized by carbonate of soda, and then feebly acidified by acetic acid, will become alkaline some days sooner than the same urine if left without admixture. The presence of a sulphite can, therefore, never be regarded as an impediment to the ammoniacal decomposition of urine and *fæces*. It is only the free sulphurous acid that possesses this property, and certainly not in a greater degree than any other free mineral acid. But as sulphurous acid is easily obtained in a gaseous form (by burning sulphur, or by treating sulphites with concentrated sulphuric or hydrochloric acid), it must be retained upon the list of useful disinfectants, especially for places that cannot be reached by fluid agents, such as inaccessible sewers. For the disinfection of linen and clothes, it is also at least as useful as chloride of lime, without being so destructive to the textures.

The carbolic acid, when used in very small quantity, prevents the ammoniacal decomposition. Properly diluted, it scarcely acts at all upon iron or zinc, and very little upon mortar; and it covers the *fœtor* of excrement completely, whilst its own odour, when diluted, is very bearable. It is not, however, readily obtainable in large quantities; it is dear, and is inferior to sulphate of iron in other respects; nevertheless, it is a good and useful disinfectant. One part of carbolic acid should be dissolved in twenty parts of water; and a quarter of a litre of this solution will be sufficient for the daily excreta of four persons, and will keep them acid until they can be conveyed away.

In order that disinfection may be of any practical utility, it must be thoroughly carried out. Partial disinfection is of no use whatever. It is merely lost labour to acidify the excreta day by day, and to let them pass into a pit containing matters already ammoniacal; and it is only when the pit and its contents have been completely disinfected that the disinfection of single evacuations can be of advantage.

The use of chloride of lime is condemned by Dr. Pettenkofer as irrational, since it is an alkaline agent. It would be paradoxical to combine it with sulphate of iron; for each neutralizing the other, no disinfection would be effected at all, unless one of the two was in greatly preponderating quantity. It might also occur that a cesspool, already rendered acid by sulphate of iron, might receive so much chloride of lime as to restore its alkalinity, so that the ammoniacal decomposition would be promoted.

Since disinfection is manifestly a prophylactic measure, it is obvious that it ought not to be delayed until the cholera has actually shown itself. When once an undoubted case of cholera has occurred in any house, disinfection is of little value there as regards the inmates, who have already been exposed to the poison; so that it will depend upon individual predisposition, and the duration of the incubatory stage,

whether more of them will be attacked. Disinfection has then its only value as regards the future intercourse that other persons may have with the place, and is useless for those who have already been there, and who, as well as the inmates, may be infected. Visitors may even carry away germs to their own houses before they show any clear symptoms of disease themselves; and these germs may be developed or not, according to local and individual predisposition. In all past epidemics of cholera, disinfection has only been commenced after the outbreak of the disease, except at the cholera hospitals of Altenburg and of the town of Zwickau, when the prophylactic disinfection with sulphate of iron was generally and in good time carried out; and where, also, especially in Zwickau, the consequences can scarcely be denied.

It must be admitted that the times for commencing and for leaving off disinfection have not yet been certainly determined; but according to past experience, the level of the surface-water affords a valuable criterion. It seems that the time when the surface-water recedes from an unusually high level is to be considered as the period of danger.

[For the disinfection of air, Dr. Badstübner (*Berlin Klin. Wochenschr.*, 1866) recommends the permanganate of potash (Condy's fluid), and especially for sick rooms. Béranger-Ferand (*Gaz. de Paris*, 1866) considers the same agent to be the best disinfectant in cholera. Both writers think it preferable to the sulphate of iron, except when its price is a difficulty in the way. Badstübner also cites cases in which it was of great value in destroying the fœtor of sputæ.

Dr. Th. Clemens, of Frankfort-on-the-Maine (*Deutsche Klinik*, 1865-6; *Schmidt's Jahrbücher*, 1866), has observed the immunity from cholera of coppersmiths. Hence he recommends as a cholera disinfectant a *spirit of chlorate of copper*, and uses the same preparation, both internally and upon the skin, as an actual preservative.

His formula is as follows: Liq. cupri perchlorati concent., ʒij.; chlorof., ʒi.; spir. vini, ʒvj. As a preservative, two or three drops of this compound, twice a day; and he also orders it to be rubbed into the abdomen. As a disinfectant, the same fluid is to be put into a common glass spirit-lamp, and the wick lighted. A vapour of chlorate of copper is formed, which in five minutes will pervade a chamber of five thousand cubic feet so completely, that all objects contained therein will be impregnated. In this way, both the air and all matters will be quickly and without danger disinfected. Dr. Clemens holds the facility with which this can be done as a great advantage; and he has never seen, even in children, the slightest injurious action from the vapour.]

ART. 3.—*The Influence of Impure Water on the Development of Epidemic Cholera.*

By Dr. NORMAN CHEVERS.

(*Indian Annals of Medical Science*, August, 1866.)

Dr. Norman Chevers supplies an instructive illustration of the influence of impure water in the development of cholera, from the pre-

sumed chief focus of the disease, the delta of the Ganges. In a valuable article on the Means of Preserving the Health of Seamen, especially of those frequenting Calcutta and the other Indian ports, he states that one of the leading Calcutta physicians has recently examined the whole line of river bank along which the shipping are moored.* This gentleman commences his report by saying that his object is to draw attention to the causes which, at certain seasons of the year, are actively engaged in destroying those who are condemned to live on this "*polluted* river," which he, not too emphatically, designates as "the Maelstrom of Death."

"His metaphor," writes Dr. Chevers, "points to that stern reality, the poison of cholera, which, discharged at various points, in the shape of sewage, upon the river bank and into the centre of the stream, pollutes the water and the air; and, added to other malignant influences common to all rivers in malarious countries at remote distances from the sea, converts a haven of refuge into a port of danger. He shows that the night-soil of Calcutta is deposited in the Hooghly, at mid-stream, at a point opposite the night-soil ghât, at the rate of 180 tons a day; that, during certain months of the year, the river-water at Cossipore, two miles up the stream, is, owing to the large quantity of sewage matter which it holds in solution, quite unfit for human consumption; and that, under the influence of an unusually high tide, the water at Barrackpore even, thirteen miles further up, becomes contaminated; and yet the river water is drunk by sailors in ships moored at various points between this reservoir of night-soil and the sea. Immediately below the night-soil ghât are a large food-market and a landing-place for boats, to which are brought rice, mangoes, &c., for the consumption of the town.

"Again, near Champatola Ghât may be seen a large quantity of stable manure and other refuse, and near Prosono Coomar Tagore's Ghât a still larger quantity, deposited with a view to filling up hollows and gaps in the bank. 'Conceive,' Dr. — exclaims, 'the loss to agriculture, the gain to the poisoned atmosphere of the harbour!' Next, the Burning Ghâts. Admitting that the process of cremation pollutes a small area only, and that those who live within that area have become habituated to, and so remain unaffected by it—[I once had the misfortune to live within that area. I became 'habituated' to it, but never ceased to be 'affected' by it. A most horrible and diabolical smoke, thick and heavy, plentifully throwing down a black greasy *snow*, and with an odour in comparison with which the fumes of burning hospital blankets are an Arabian perfume]—still a pernicious influence must extend to the ships lying to leeward, when the wind blows over them from these centres of incineration of human flesh.

"Then the sewers. Of these Dr. — counted twenty-two between the ghât opposite the Chitpore Dispensary and Hastings Bridge, a distance of four miles. Between these points are the principal moorings of the harbour. There are others lower down, such as the 'Kidderpore' and 'P. & O.' moorings, &c.

* "Cholera in the Port."—*The Indian Medical Gazette*, July 1st, 1866.

“The mouth of each sewer opens directly upon the commencement of the soft muddy bank, on the irregular surface of which much of the contents is retained; and in February, March, April, May, and June, the poison of cholera, from this source, is in active operation. It is worthy of note that a sewer empties itself close to each of the principal bathing ghâts. As if the sacred stream was not already poisoned enough by the daily Augean deposit of night-soil, the bathers are furnished with a special fountain for themselves. Some of the sewers have acquired great notoriety, and it has long been known that the ships moored near them have sent more cases of cholera to the hospital than others which have been moored elsewhere. ‘It is idle,’ Dr. ——— exclaims, ‘to talk of unripe fruits and bad lemonade; of Jack’s imprudence in exposing himself to the sun; of his drunken habits, &c., and then to add—no wonder he gets an attack of cholera! Doubtless, all these have their influence in predisposing him to the disease, or in evoking it; but the *fons et origo* remains, and we *must not* continue to shut our eyes to the fact.’ He then cites Mr. Hugh Macpherson’s observations, and goes on to show that, of 392 cases of cholera which have been admitted into the Medical College Hospital during the three years ending in 1865, 189, almost half, have come from the ships in the harbour. Of the sailors who live on board their ships, 3 per cent. annually are taken to the hospitals; whilst, of those who live in Lal Bazaar and its purlieus, 20 per cent. are sent there. Of the admissions from the river, *two-thirds* are from cholera, whilst of those from the shore only *a tenth part* are attributable to that disease! Of the 189 cases sent from the river to the Medical College Hospital, the Esplanade moorings have supplied 34 per cent., the remaining 66 per cent. being brought from *twenty-five* other moorings. During the same period 303 cases of cholera have been received into the General Hospital from ships. Of these, 15 per cent. have come from the ‘Esplanade,’ and 17 per cent. from the ‘Cooly Bazaar’ moorings, whilst the remaining *sixty-eight* have been distributed over *twenty-six* other moorings! Very few cases are taken to the Medical College Hospital from ‘Cooly Bazaar’ moorings, owing to the great distance. The cases which occur in them are sent to the General Hospital. Cases from the ‘Esplanade’ moorings, on the other hand, are principally taken to the Medical College Hospital, it being the nearest of the two. It thus appears that the ‘Esplanade’ and ‘Cooly Bazaar’ moorings are amongst the favourite haunts of cholera. It is urged that crowding may partly account for these results. But we find that the ‘Prinsep’s Ghât’ moorings, where, in Dr. ———’s recent excursion, there was a greater crowd of ships than elsewhere, have, during the period under review, sent only nine cases of cholera to the General and Medical College Hospitals, the ‘Esplanade’ moorings having sent 94, and the ‘Cooly Bazaar’ 61. The former of those two is now the most prolific in cholera-poison; the latter, though still pregnant with it, having materially improved in this respect during the past few years. The comparative immunity of the ‘Prinsep’s Ghât’ moorings from cholera may be due (and although Dr. ——— is not prepared to state it as a fact, he believes that it is so) to the sewer which opens into the river opposite to these moorings, conveying only surface-drainage. He believes that conservancy laws have been brought

to bear upon the 'Cooly Bazaar' locality generally; and that great improvements have been made in the river bank at this point. The result is encouraging, and should lead to further endeavour."

ART. 4.—*On the Trichina and Trichinosis.*

By M. DELPECH.

(*Annales d'Hygiène Publique*, Juillet, 1866.)

In an elaborate report on various papers on trichinosis, communicated to the Academy of Medicine, Paris, and from a review of the whole subject, M. Delpech arrives at the following conclusions:—

"Although the symptoms and gravity of trichinosis had been fully known only since the year 1860, still the disease was by no means a recent one, and its existence in Germany at a remote period, in an epidemic form, could be readily demonstrated.

"It was then confounded with various other affections, and was more especially looked upon as a peculiar and exceptional variety of typhoid.

"The disease has since given rise to much arduous research, and can scarcely in future escape detection, when it has been attentively watched in every stage of its development.

"Disturbance of the digestive organs followed by œdema of the face, and subsequently by severe muscular pain, and by a degree of dyspnoea which may even end in asphyxia on account of the impossibility of the movements of respiration, is an aggregate of symptoms not to be met with in any other affection. These morbid manifestations correspond with the successive birth in the digestive tube, and of the passage into the muscular structures of trichinæ in numbers sometimes enormous, but in general proportionate to the quantity of parasites which have been swallowed. Their presence can be demonstrated during life by the microscopic inspection of a minute particle of muscle removed from the patient's person with peculiar instruments, and by an innocuous and almost painless operation. In doubtful cases, the diagnosis can, therefore, at a certain stage of the disease, be confirmed by direct inspection.

"In general, one tainted animal will infect many persons. Hence more or less widely-spread and severe epidemics, according to the condition of the animals, the variable quantity of the flesh consumed, and the mode of cooking adopted.

"Certain animals are, as well as man, liable to trichinosis. In carnivora and omnivora the complaint occurs spontaneously, and herbivora may also artificially become affected, but only by the intervention of the human subject.

"In man the disease arises from the consumption of raw or insufficiently cooked pork flesh, tainted by the presence of trichinæ.

"In pigs the propagation of the parasites is referable to several causes. They eat trichinized animals, especially rats, dead or alive, or abandoned on dung-hills or in fields. They feed on human excrement,

or on the dejecta of pigs which have recently consumed trichinized flesh, and which excrete, with the contents of their intestines, fecundated female trichinæ. Moles, earth-worms, the larvæ of flesh-flies, the beet-root worm have nothing to do with the transmission of trichinæ.

“When the disease occurs spontaneously in pigs, it seldom gives rise to characteristic symptoms, and microscopic inspection alone leads to the knowledge of the parasites. In the human subject, the cyst, when encrusted with calcareous salts, can easily be discerned with the naked eye, in the shape of white patches, and the microscope affords further conclusive evidence. In the countries where trichinosis prevails, this mode of examination has become a general precaution, whether carried out by individuals or by order of the Government.

“Merely optional microscopic examination, although doubtless useful, can give no absolute security, on account of the necessary absence of regularity and supervision. Compulsory examination alone can yield any seriously beneficial results. Two objections are urged against it—viz., the difficulty of carrying it out, and the uncertainty of the information supplied in cases in which the animals are but slightly affected. These are, it is true, serious considerations, but nevertheless the advantages derivable from compulsory microscopic inspection are such that the measure should unhesitatingly be adopted in all countries contaminated by trichinosis.

“France appears hitherto to have escaped the contagion, and no cases have yet been adduced of acute or encysted trichinosis, nor have any records been brought forward of former epidemics, as in Germany. The rats of the slaughter-houses do not seem to have been infected; at least, not habitually. The immunity is to be traced to the different customs of both countries, and to the more complete boiling to which the meat is submitted in France, which checks the development and propagation of the parasites.

“A temperature of 75° Cent. (167° Fahr.) alone can secure the destruction of the trichinæ. The same result may be attained by thorough and protracted salting, or by a hot fumigation of twenty-four hours’ duration. Cold smoking does not destroy the worms.”

M. Delpech further submitted the following resolutions to the approbation of the Academy:—

“1. The apprehensions awakened in France by the epidemic of trichinosis in Germany have not hitherto been justified by any facts observed in this country.

“2. The custom prevalent in France of thoroughly boiling pork flesh explains this immunity, and should be more than ever persevered in.

“3. As no epidemic, and even no isolated cases of trichinosis have been observed, it is unnecessary to resort to any special measures of public hygiene, or to recommend the adoption of a general compulsory microscopic examination of pork flesh.

“It might nevertheless be useful to establish a service of inspection in certain towns provided with public slaughter-houses, with a view to ascertain by authentic returns the existence, the absence, or the proportion of trichinosis observable in the porcine race.

“4. Certain conditions of rearing and feeding being calculated to influence considerably the development of trichinosis amongst pigs,

it would be well to distribute in the agricultural districts circulars or tracts for the promulgation of the precautions to be adopted with a view to the preservation of the animals."

These resolutions, drawn up by the reporter, with the assistance of M. Raynal, were adopted without discussion.

ART. 5.—*On the Influence of the Volcanic Emanations at Santorin upon the Health of the Population and Vegetation.*

By M. DA COROGNA.

(*Archives Générales de Médecine*, Août, 1866.)

In a note addressed to the Academy of Sciences, Paris, M. Da Corogna stated the following respecting the influence of the recent volcanic eruption in the roads of Santorin, upon the inhabitants and vegetation of the island:—

1. The eruption exercised a manifest effect upon the health of the people.

2. It gave rise particularly to inflammation of the conjunctivæ and fauces, bronchitis, and disturbance of the digestive organs.

3. The acid ashes were the direct cause of the inflammation of the conjunctivæ, while the other disorders were to be attributed to the sulphurous acid.

4. Plants also suffered from the eruption, especially the Liliacæ. This was probably due to the hydrochloric acid diffused at the commencement of the eruption. On the other hand, the hydrochloric acid emanations appear to have exercised a beneficial influence upon the vine disease, owing, perhaps, to the destruction of the oïdium by them.

ART. 6.—*Impure Water as a source of Malaria.*

By Mr. D. J. MOORE, L.R.C.P., Marwar Political Agency.

(*Indian Annals of Medicine*, August, 1866.)

Mr. Moore, from an inquiry into the truth of the opinions generally ascertained regarding malaria, argues that there are not wanting special instances of individuals in whom the use of impure water has been immediately productive of malarious affections. Dr. Mackinnon has mentioned cases of this description.* Similar occurrence have fallen under his own observation. Mons. Boudin records that, in 1834, eight hundred soldiers embarked at Bona in three transports. On board one ship ninety-eight men suffered from remittent fever. On inquiry it appeared the water had been taken from a known malarious locality. The ships

* The Author's *Health in the Tropics*, p. 66.

supplied from other sources did not furnish one case. Nothing, Mr. Moore thinks, appears stronger than both the negative and positive evidence on this point. At the same time, continental writers, as Hinke and Grooz,* assert, marsh water may be taken in Holland and Hungary without danger; but the latter-mentioned writer remarks, "It is customary to mix some kind of spirit with it," which practice probably tends to neutralize the noxious principle. In India, the only tank or stagnant water which may be safely drunk, without previous filtering, is that covered with a sort of green weed, something resembling "duckweed," and belonging to the *Lemnæ* order. This growth imparts a greenish colour to the water, and renders it rather unpalatable. It, however, possesses a remarkable filtering power, attracts numbers of animalculæ, which would otherwise disport in the clear fluid beneath, and be liable to be taken away and introduced into the human system. The natives of most parts of India are perfectly aware of this provision of nature, and drink with great caution any tank water where this weed does not exist. An anecdote illustrating the foregoing is told of Sir Charles Napier. When Commander-in-Chief, at an inspection of a native regiment in the Punjab, he observed the *bheesteers* drawing water from a foul-looking tank, covered by a slimy greenish weed. This was ordered to be cleared away, the result being that the water soon turned putrid, and it was not until a fresh crop had grown that it became wholesome and drinkable. The precise manner in which this weed or plant acts in purifying water is not known, but it probably evolves oxygen, which may exert some beneficial influence. It is stated that fishes even die on the removal of all vegetation from ponds; and the utility of a piece of moss, introduced into the basin where gold-fish are kept, is well known. Aquatic weeds have been imagined to destroy malaria in the same manner as trees have been supposed to do.

"In the course of this paper," Mr. Moore continues, "it has been shown that malaria is not the product of either swamps, marshes, drying-ground, or decaying vegetation. It is also demonstrated with equal certainty, that if malaria is evolved from the earth, it must be formed in surfaces of the most varying composition. It must, moreover, be evident that the generally-received characteristics of malaria are not in accordance with the so-called facts usually quoted in their support. Lastly, various authorities have been named, who more or less strongly disbelieve in the existence of malaria. To deny the existence of a poison capable of inducing intermittent in the human system is, however, I think, incompatible with what we know of other maladies. No one, for instance, would, at the present period (although attempted in years gone by), dispute the formation of hydrophobia poison. Typhus fever also, together with typhoid, variola, scarlatina, syphilis, and many other diseases, are all characterized by their peculiar phenomena; and all are caused by distinct poisons. The conclusion, therefore, that a specific malarious poison exists is not only reasonable, but inevitable. But that this deleterious agent is formed in, and exhaled from, the whole surface of the earth is fairly open to doubt. It is,

* Quoted by Parkes, *Pract. Hygiène*, p. 52.

indeed, questionable if malaria exists in the atmosphere at all. There are, perhaps, more cogent arguments in favour of water being the sole medium by which malaria is introduced into the system. The decline of ague in Scotland, England, and other European countries, has been contemporaneous with the provision of better drinking-water. When Englishmen maintained hundreds of 'stew-ponds,' to supply the hebdomadal fish diet; when, instead of carpets, fresh green rushes were placed on the floor every morning; when, in the absence of glass windows, the night air penetrated freely into human residences; when agriculture was rude, and draining unknown, our forefathers suffered severely from ague, fevers, and dysentery. But the 'stew-ponds' could not have produced these diseases, as to preserve the fish they would be maintained full of water, and malaria is said only to arise from foggy and drying-land. Rushes on the floor would not give rise to ague; for, as already demonstrated, the disease occurs in localities entirely destitute of vegetation, either living or dead. With regard to drainage, Dr. Christison has proved that ague ceased in Scotland before this measure became general. It is, however, certain that at the period when Englishmen and Scotchmen suffered from ague, then water supply was most impure, and unfit for the purposes of human consumption. It is also equally true that malarious affections disappeared from the land as the introduction of pumps superseded the use of the old, open draw-well, or still more objectionable supplies.

"But however impure the water may have been in European countries, it can scarcely, even at the worst, have attained to the minimum condition of defilement prevalent in India. I write more particularly of the Moffussil. Here wells are almost invariably open, and frequently the only water obtainable is from the village pond. Leaves of trees, dust, dirt of every description, and frequently both dead and living insects and reptiles may be seen floating on the surface. And as a climax, village tank-water is defiled by the solid and fluid *excreta* of the hundreds of buffaloes and kine which twice a day are brought to drink; and also by the ablutions, and not unfrequently by the *excreta*, of the villagers themselves! I have, indeed, many times seen the latter fill their water-pots within six feet from a micturating buffalo! Our own drinking-water is even now carried in skins very imperfectly tanned, the interior of which can never be cleansed; and it is too often cooled in dirty jars. But water from a tolerably pure source, even when maltreated in this manner, would be taintless, when compared with that drawn from village tanks. I quote from a recent official report.* 'When one remembers that this tank water washes down from the fields a great proportion of the ordure, the remains of dead animals, and every conceivable filth that accumulates in the environs of a village, and that shallow and unrenewed, it remains stagnant and reeking under a tropical sun, it does not seem strange that the people who have no other drink going into the water, and before filling their vessels washing their legs and feet in the very water they were going to drink. We took some

* By Mr. Campbell, Sup. of Police, Cauara.

of the water out in a tumbler, and found it of a yellow colour, and so thick that we could not see through it.' On being submitted to examination, the water was found to contain a large *percentage* of organic matter, and numerous animalculæ of the protozoic variety. And here it may be mentioned that, while the air over marshes, when submitted to chemical experiment, does not materially differ from that of other places, marsh or malarious water always contains a large amount of vegetable organic matter. From 10 to 40 or 50 grains per gallon is not uncommon, and in some instances there is more. And this may probably be the poison which excites malarious fever. It is certainly more consistent with our general knowledge of these matters to imagine that the fever, which so frequently devastates Indian Moffussil districts, is due to the impure water consumed, rather than to an invariable poison in the air, which, arising from the whole surface of the land, is indestructible by the otherwise all-powerful oxygen. And this view is, moreover, strengthened by the immunity from fever which those appear to possess who scrupulously avoid drinking impure water, and who confine themselves to soda-water when travelling in the Moffussil. Further research, experiment, and discussion, regarding the cause of malarious fever, is certainly demanded from medical officers in India."

ART. 7.—*Influence of Sewing-Machines on the Health and Morals of Workwomen.*

By M. GUIBOUT.

(*Journal of Practical Medicine and Surgery*, July, 1866.)

Any one who has witnessed the wonderful activity of that ingenious contrivance, the sewing-machine, worked by intelligent seamstresses, would scarcely imagine that it is calculated in some degree to endanger the morals of the workwomen. This, however, appears to be the case, if we can trust a recent communication of M. Guibout to the Medical Society of Hospitals.

"A month ago," says this learned practitioner, "two women, entirely unknown to each other, and employed in different factories, applied to me for advice on the same day.

"The first was a pale, lymphatic subject, with hollow discoloured cheeks, much bent and emaciated, and suffering from severe epigastric pain, dyspepsia, incessant leucorrhœa, and a marked state of general debility. She attributed all her symptoms to the effects of the sewing-machine. Before her admission into the factory, where she was in the habit of using it, she was strong, fresh-coloured, in vigorous health, and had never been affected with fluor albus. Since seven or eight months she had observed a gradual decay in her health, strength, and appearance, and leucorrhœa had set in, growing daily more and more copious. 'Nor am I,' said she, 'the only sufferer; several other young women in the establishment are similarly affected from the same cause. The constant action of the lower extremities, and the see-saw motion of the

entire body, wear them out, and cause pains in the back and stomach, and a leucorrhœal discharge.' She further stated that she had not noticed in her own case any peculiar sexual excitement, but to this she was not liable under any circumstances. It was not so with several of her companions, who were often compelled to discontinue their labour, and to have frequent recourse to lotions with cold water.

"My second patient was a strong, dark-haired woman, of sanguineous temperament. She worked a sewing-machine in a very extensive establishment, and had enjoyed perfect health up to the period of her admission into the factory, where she remained about a twelvemonth, being obliged to leave on account of the fatigue and pains caused by the nature of her occupation. She also alluded to the excitement of the sexual organs. 'Five hundred women are employed in the house,' said she, 'and two hundred at least experience the same effects as myself. The workwomen are constantly changing, and are unable to persevere at the trade for any length of time. They enter the house in good health, but all leave it thin and debilitated.'"

Dr. Thibault had already made the same remark, and although, in the short debate which followed, M. Vernois expressed his opinion that the disadvantages complained of were exceptional, it is now sufficiently demonstrated that these facts are by no means uncommon, and it is therefore highly desirable that mechanical skill should devise for sewing-machines some other motive power than the workwomen's feet.

[Dr. William Ord, who made, in 1863, an investigation into the sanitary condition of dressmakers and needlewomen in London for the Medical Department of the Privy Council, arrived at a conclusion different from that of M. Guibout and Dr. Thibault on the effects of the sewing-machine upon the health of the workers. He says (*Sixth Report of Medical Officers of the Privy Council*, p. 381): "I have found that the influence of the sewing-machine is, for the most part, beneficial to the workers. In the first place, and this is probably the essential point, they earn far more than the needle-workers. Even where the payment is for the time the machinists are paid half as much again as the needle-workers, and at piece-work the difference is still greater. In the second place, the muscles of the limbs and trunk are kept in exercise—a circumstance which, associated with the power of getting good food, appears to tend to improve the health of the machinist. On the other hand, the cramped position rendered necessary is alleged to cause pain in the chest and indigestion; and the continual noise made by the machines is said, in some cases, to cause headache and giddiness. But touching the important fact that, comparing in a given form the machinists and the needlewomen, the comparison, as regards healthfulness of appearance, is greatly in favour of the machinists, there can be no doubt."

More recently, Dr. Down, Superintendent Physician of the Redhill Asylum for Idiots, has made some observations confirmatory of M. Guibout's conclusions. His attention had been attracted by the constant recurrence of cases characterized by pallor, lassitude, pain in the back, leucorrhœa, and excitement of the sexual organs. He was led to investigate the employments of these patients, and found that they almost invariably worked habitually at the sewing-machine, and

that they were chiefly employed in the manufacturing work which is carried on in the east end of London, and which is done by machines worked with treadles moved alternately by the feet. These machines are heavy, and require considerable exertion on the part of the operator.—*The Lancet*, Oct. 20th, 1866.

Dr. Down's and M. Guibout's observations refer to special evils arising from the use of the sewing-machine; Dr. Ord's to the comparative health of machinists and needlewomen.]

ART. 8.—*Impure Water as a cause of Typhoid Fever.*

By Dr. SEATON.

(*The Lancet*, October 20, 1866.)

Page-green is a detached hamlet in the parish of Tottenham. It numbers some eighty or ninety houses, the oldest of which date only eleven years back; the newest have their newness still fresh upon them. Certain nuisances to which this hamlet was subjected, one in especial arising from the distribution of the sewage of Tottenham over the land in the immediate vicinity, recently led to an appeal being made for relief to the Privy Council. Dr. Seaton was instructed by the Council to make an investigation of the medical aspects of the question, and his inquiry brought to light certain most interesting and instructive facts.

It would appear that typhoid fever and diarrhoea have prevailed in Page-green during the last two years to a remarkable extent, but with little fatality. In particular, especial attention was directed to an outbreak which began in 1864, became greatly extended by Christmas in that year and during the spring of 1865, and has continued to the present time. From information which Dr. Seaton obtained through the local practitioners (Dr. May, Dr. Pool, Mr. Hall, and Mr. Wolstenholme), as well as from a personal examination of fifty-six houses, it was ascertained that there had been a case or cases of fever in twenty-nine. Nearly sixty cases of typhoid fever were vouched for on medical authority, and Dr. Seaton satisfied himself that the number of cases which had taken place in the hamlet was not short of a hundred. Four deaths only from this cause had been registered in the sub-district; but this slight mortality is accounted for by the majority of the cases having been young children.

In seeking for the cause of this continued prevalence of typhoid fever, attention was in the first instance directed to the supply of drinking-water. The houses inspected derived their water from two sources—surface-wells or the water-works of the Tottenham Board of Health. None of the houses had on the premises a supply from both sources. An inquiry as to the effect of this difference in the source of water-supply gave the following results:—

Of twenty-four houses deriving their drinking-water from surface-wells, a case or cases of fever had occurred in nineteen; in seventeen there had been more than one case of fever; in thirteen, a succession of

cases of fever or diarrhœa; and the approximate total number of fever cases, including diarrhœa, had amounted to eighty.

Of thirty-four houses deriving their supply of drinking-water from the water-works of the local Board of Health, a case or cases of fever had taken place in ten; in four there had been more than one case of fever; in two, a succession of cases of fever or diarrhœa; and the approximate total number of fever cases, including persistent diarrhœa, did not exceed twenty-two.

"The difference," writes Dr. Seaton, "was so striking as to lead at once to the further inquiry whether, in the houses supplied by the local Board in which fever had prevailed, the inhabitants had really taken all their drinking-water from the Board's supply, or whether they might not have derived at least some share of it from their neighbours' surface-wells. I found that the occupants of six of these houses—including all the four which had had more than a single case of fever—from dislike of the Board's water, or from the irregularity with which they were supplied, very frequently borrowed water from their neighbours' wells; at three of the houses, from removal of the families in which the fever case had occurred, I could get no information on this point; at the remaining house, at which there had been last autumn a case of mild typhoid in a child, the water drunk was, so far as could be ascertained, exclusively that of the local Board. Excepting these last four cases (concerning three of which no information on the subject could be got), it is established that in every case of fever which has occurred at Page-green the supply of drinking-water had been wholly or in part, and in the immense majority of cases wholly, from the surface-wells."

The wells are described as being very shallow—about seven or eight feet deep, in porous gravel. Although the water they yield is said to be generally bright and pleasant, it was shown to be liable to surface and sewage pollution, becoming at times unpleasant to the taste after heavy rains, or from matters thrown down the sinks. In one instance, some carbolic acid having been thrown down the yard sink, it was distinctly smelt in the water pumped from the well. A chemical analysis of certain specimens of the water, made by Professor Miller, showed that it contained a very unusual amount of ammonia as well as organic matter.

"The way," observes Dr. Seaton, "in which people go on unconsciously drinking in fever and diarrhœa from such wells as these is so aptly illustrated by the occurrences at Caine's-terrace, that I must be permitted to give in brief detail the results of my inquiry at the five houses of which the terrace consists, all deriving their water from two or three surface-wells. In the one of them from which the water was taken for analysis, Mr. Gaffney has lived for three years; the family consists of four. All had diarrhœa soon after they came, and in November, 1864—January, 1865, the son and daughter had marked and severe typhoid; the son narrowly escaped dying of it. They were attended by Dr. May. In the next house, supplied by the same well as Gaffney's, in December, 1864, Mrs. Greenwood was attacked with typhoid, and was attended by Dr. Pool. In May, 1865, the present occupant, Mrs. Slark, came. She, her son, and her daughter (the only permanent inmates) had diarrhœa immediately on their coming; the lodgers whom she received have, each as they came, successively had diarrhœa, and

some of them have also had low fever. Mrs. Barnes has lived in the next house for two years; they are four in family, and for the first six months of their residence they had nothing but fever and diarrhœa. Lately they have begun to take lodgers; they have had two sets, and each set has had diarrhœa. The two remaining houses are occupied by families who have only resided for seven weeks, but in each of these houses there was fever last year. Of the present occupants, the family residing in one of them, five in number, have all suffered from diarrhœa since they came. The family residing in the other, three in number, have not suffered in any way; but *this family boil first all the water which they use for drinking*. Mrs. Slark informed me that many of the neighbours who were supplied by the local Board came begging water from her well; and I found that at two houses so supplied close by—the only two houses supplied by the local Board in which there had been succession of cases of fever and diarrhœa, viz., Graham's and Bartholomew's—water had very frequently been borrowed from this well."

The story of Caine's-terrace was repeated in another row of houses.

From the history of the entire outbreak and the circumstances stated, Dr. Seaton thinks that the conclusion is irresistible, "that the fever and illness at Page-green have been mainly kept up by the use of impure surface-water for drinking;" and he deemed it "a matter of the most urgent necessity that the surface-wells should at once be closed."

A local belief that the prevalence of fever was connected with the sewage irrigation was shown to be erroneous by the fact that there was no irrigation until April, 1865, a period long subsequent to the appearance and diffusion of the disease."

Mr. Morgan, C.E., who was specially instructed by the Privy Council to investigate the drainage, sewage irrigation, and water-supply of Page-green, states of the drainage, that all the houses, with the exception of three that have cesspools, drain into the sewers of the local Board; that where sinks are inside the dwellings they are connected with the sewers; and that no surface-drainage, either from the back premises or roads, is permitted to run into the sewers.

All the houses in Page-green, Dr. Seaton states, have water-closets communicating with sewers, except the three referred to by Mr. Morgan; but in those houses which derive their water-supply from wells, there are no means of flushing except by pails of water poured down occasionally.

ART. 9.—*On Animal Vaccination.*

(*Journal of Practical Medicine and Surgery*, May, 1866.)

In the discussion in the Academy of Medicine, Paris, upon M. Depaul's report on animal vaccination, an abstract of which document was given in our last volume, the views of the reporter received a severe check.

Vaccination from arm to arm is obviously traversing a critical period, which began with the doubts expressed as to its preservative virtue

and with the accusation of having been instrumental, under very exceptional circumstances it is true, in propagating syphilis.

To obtain strong, unadulterated vaccine-lymph, has been the object of those who, without questioning the undoubted utility of Jenner's discovery, and unwilling to revert to inoculation of small-pox, have sought in cow-pox, or pustular grease, the means of preserving mankind from the perils of variola. On several occasions, hopes have been entertained that it might be possible to regenerate human virus by inoculation from animals, but the lymph thus obtained soon deteriorated, and it became necessary to revert to vaccination, a perfectly legitimate and highly beneficial procedure. All practitioners at present agree, M. Depaul as well as M. Bousquet, that the vaccine matter now produced is neither more nor less powerful than it was in Jenner's time, and that the condition of the human system alone is answerable for the occasional inefficacy of the lymph. The question now agitated entirely applies to the purity of the vaccine matter, and to the expediency of persevering in vaccination from arm to arm, or of exclusively using the lymph supplied by animals and especially by cows, who hitherto seem to enjoy perfect immunity from syphilis.

In order to solve the problem, it is in the first place indispensable to ascertain the precise value and power of the lymph derived from heifers at Naples and Paris, and whether it is constituted by cow-pox, traceable by uninterrupted succession to natural cow-pox, or by morbid secretions artificially obtained by the inoculation of human vaccine matter to the cow. Now, it is quite true that M. Palasciano of Naples, stated at Lyons, where he had proceeded for the purpose of describing the advantages of the method since introduced into Paris by M. Lanoix, that the lymph supplied by his cows descended in straight line from originally natural cow-pox, and it is equally true, that M. Lanoix imported from Naples a heifer vaccinated with this lymph. But previously to, and since that date, it is not certain that the natural cow-pox, having accidentally become exhausted, human vaccine matter was not inoculated to a cow, and propagated afterwards from one animal to another. At the Academy, Messrs. Bousquet and Jules Guérin openly asserted that such was the case, and M. Lanoix has not positively denied the statement. We are, therefore, justified in assuming that the pretended cow-pox which was so much talked of last summer was merely human vaccine matter inoculated to the cow, and consequently destitute of the guarantees it was supposed to possess.

We should add that M. Depaul has not persisted in his vindication of M. Lanoix's method, but has consistently instituted a search for genuine natural cow-pox: and having ascertained that at fifteen miles from Orleans a heifer, aged two years and a half, presented the characteristic pustules, he has ordered a second cow to be inoculated with the lymph supplied by the first; the experiments necessary to an impartial appreciation of the merits and superiority of animal vaccine matter will be therefore carried out with the lymph derived from this source, obviously free from all taint referable to the human subject.

ART. 10.—*Poisoning by Silk Thread.*

By M. CHEVALLIER, jun.

(Moniteur d'Hygiène et de Salubrité Publique ; Journal of Practical Medicine and Surgery, June, 1866.)

The silk thread employed by seamstresses is liable to acquire poisonous properties in consequence of a fraudulent practice described as follows:—

“The value of the best quality of silk varies from sixty to seventy francs a pound, and the material is sold wholesale by weight. For many years it has been the custom to increase the weight by steeping the silk in sugar and water, or in an infusion of gall-nuts; but this fraud not being found to yield sufficiently large profits, a patent was taken out for another plan, which consists in soaking the silk, whatever its colour, in a bath of acetate of lead, and after drying the skeins, exposing them to a current of hydrosulphuric acid. The result is the deposition of a quantity of sulphuret of lead which greatly adds to the weight of the thread, and, therefore, to its mercantile value. We are acquainted with a person at the head of an extensive dressmaker's establishment who, from the use of silk thread thus prepared, was attacked as well as her workwomen with painters' colic; some of the women even lost their teeth, in consequence of their habit of biting off the ends of the thread, an operation during which they absorb a portion of the lead attached to it.

“The following is an easy method of discovering the fraud which is sometimes carried so far that some silks have been found to contain as much as 23 per cent. of their weight of sulphuret of lead: place a few threads at the upper part of a tube closed at its inferior extremity, and moisten them slightly with water containing a small amount of acetic acid or strong vinegar. When the silk is impregnated, add a few drops of a solution of iodide of potassium. If any lead be present, a golden deposit of iodide of lead will at once betray the adulteration; and the weight of the iodide formed, and that of the silk before and after the operation, drying included, affords a clue to the quantity of lead introduced to deceive the purchaser; a dangerous kind of fraud, inasmuch as the action of the poison is slow and insidious, and entails injury to the teeth, general intoxication of the system, paralysis of the intestines, and may even cause death.”

ART. 11.—*Cholera Hospitals.**(The Lancet, July 28, 1866.)*

A report on Cholera Hospitals has been drawn up by the Council of the Epidemiological Society, of unusual interest. In March, the Council, having regard to the threatened outbreak of cholera, drew up a series

of queries referring (1) to the advisability or not of cholera patients being admitted into the general wards of hospitals; (2) to their admission into special wards set apart for them in hospitals; and (3) to the necessity of special hospitals for the treatment of such cases. These queries were distributed amongst the most eminent members of the profession, and a series of replies of great value have been received. Amongst those who have expressed opinions in answer to the queries may be enumerated the Presidents of the Royal College of Physicians and of the General Medical Council, the Directors-General of the Army and Navy Medical Departments, the physician to the Secretary of State for India in Council, Dr. Jenner, F.R.S., Dr. Milroy, Professors Laycock, Parkes, Maclean, Gairdner, Sir D. J. Corrigan, and numerous other distinguished men. The following is the substance of the report:—

I. There appears to be a general concurrence of opinion, expressed or implied, that under certain circumstances and conditions cholera is liable to be communicated from person to person; the liability being usually in proportion to the crowding of many persons together, the defective ventilation of apartments, and the neglect of thorough cleanliness in respect of person or abode.

In addition to the possible risk of the extension of the disease from this source, the alarming character of the symptoms, and the necessity for unremitting attendance upon the sufferers, are calculated to produce terror in the minds of spectators, and thus strongly predispose them to be attacked during an epidemic season.

For these reasons the opinion is very generally held that it is unadvisable that cholera patients should be admitted into wards which are occupied by other sick inmates.

The experience, however, of some of the metropolitan hospitals in past epidemics shows that, due attention being paid to sanitary arrangements, cholera patients may be received, in limited numbers, into the general wards without injurious results either to the other sick or to the ordinary attendants.

No instances have been referred to, in the evidence before the Council, in the opposite direction—viz., of the disease having spread to the other inmates of a ward in a well-regulated hospital.

II. With respect to the second query, the experience of the metropolitan physicians who have favoured the Council with replies appears to be that, with proper precautions, cholera patients may be admitted into separate wards in general hospitals or infirmaries without undue risk of the extension of the malady to the other inmates of the institution.

This opinion is shared by all the respondents who have had experience of the disease in tropical countries.

It would have been very desirable to have been informed of the results on this point in some of the military and naval hospitals in this country and also abroad.

The precautions above referred to are these:—(a) Ample space to each patient; not less than 1500 or 2000 cubic feet. (b) Thorough ventilation of the wards at all times, both night and day. (c) Immediate disinfection and removal of the excreta, soiled linen, &c. (d) A separate staff of nurses.

III. The reply to the third query depends much on the opinion

formed in respect of the two former questions. If cholera patients are not admissible into general hospitals or infirmaries under any conditions, it is obvious that some extemporized and special arrangements must be provided for the reception of the destitute when attacked.

But even when they are admitted, there are various circumstances in which it will be advisable or necessary that special hospitals should be provided, *e.g.*—(a) When general hospitals or infirmaries are at a distance from the seat of the actual or apprehended outbreak. (b) When there is a want of accommodation, with due regard to the ordinary patients, or when the accommodation is unsuitable or objectionable.

In selecting the site of special hospitals, the following points require to be attended to:—(a) Nearness, if possible, to the chief seat or seats of the outbreak. It is important that cholera patients should not have to be carried far. There is, moreover, great risk in moving patients in, or verging to, the state of collapse. (b) Airiness, and freedom from intrinsic or contiguous sources of atmospheric pollution. (c) A dry soil and raised situation are, of course, to be always preferred to a low and damp one.

Amid the crowded districts of a large town, it appears preferable that several small and suitable hospitals, or “houses of recovery,” should, if possible, be established in different localities, rather than one or two large hospitals for the reception of a great number of cholera patients.

The remark that the presence of an experienced staff of medical officers in general hospitals, and the existence of more complete appliances of every sort in them than is likely to be provided in extemporized special hospitals for the treatment of cholera patients, are marked advantages in favour of the former, deserves consideration.

The general conclusions of the Council are these:—

1. That it is, on the whole, unadvisable that cholera patients be admitted into the ordinary wards of general hospitals or infirmaries.

2. That cholera patients can be safely admitted into special wards in general hospitals, due precautions being taken; and therefore that it is desirable, as an important means of providing accommodation for the destitute when attacked, that the authorities of these institutions grant this valuable benefit to the public.

3. That it will be often necessary that special hospitals be provided in aid or in lieu of general hospitals and infirmaries.

In addition to these arrangements for the accommodation of the poor when attacked with cholera, the Council would recommend that places of refuge be provided for the temporary sojourn of some of the unattacked inmates of unwholesome dwellings and localities where the disease has appeared.

ART. 12.—*On Scurvy in the Merchant Navy.*

By Dr. W. DICKSON, R.N., Medical Inspector H.M. Customs.

(Proceedings of the Epidemiological Society, June 4, 1866.)

The author had been engaged during the last twenty months in a series of investigations instituted by the Board of Trade, with the view of ascertaining the origin of scurvy in various ships which had arrived in the port of London, and in which the cases of the malady were either so numerous or so flagrant as to demand the notice of the Government.

Three years ago the Medical Officer of the Privy Council had called attention to the discreditable fact that this most easily preventible disease was still extensively prevalent in merchant ships, and at his request Dr. Barnes drew up a very valuable report, founded on his experience in the *Dreadnought*, where examples of scurvy are always to be found, and in every degree of intensity.

The public is chiefly indebted to the officers of that excellent hospital for information on the subject, from Dr. Budd, thirty years ago, to Mr. Leach, the present Resident Physician, who has been indefatigable in exposing the abuses which, as he has gleaned from the narratives of the patients, indubitably exist, and in pointing out the means of redress. A very unsatisfactory feature in the recent history of scurvy is that, notwithstanding the acknowledged improvement in the condition of our merchant seamen since 1854 (the date of the Shipping Act), the disease appears to have increased rather than diminished.

The number admitted, for example, to the *Dreadnought* last quarter was 39; that for the corresponding period of the three previous years being 22, 20, and 15. The admissions annually for some years have averaged about 90; those at Liverpool about 50. But these, of course, are only the more severe cases, hundreds of others not coming under professional observation, or only casually. At the Poplar Sailors' Home, for instance, it is reported that one-half of the inmates are affected in some degree, and one-twentieth are seriously afflicted.

In the scurvy-stricken ships the proportion of sick is often very large, ranging from 9 to 90 per cent. on strength. The mean ratio of 26 ships, with crews averaging 20 in number, was four cases, or 20 per cent. Out of 55 cases received from British ships into the *Dreadnought* 14 belonged to London ships, 21 to Liverpool, and 20 to other, chiefly northern, ports. Of the foreign vessels, those from Hamburgh appear to be most affected. The voyages chiefly productive of scurvy are those from India and China, the duration of which varies from 90 to 150 days. The disease seldom breaks out in less than sixty days from leaving port. Any gross defect in the diet of the crew will therefore show itself unequivocally in the last days or weeks of the homeward voyage. It is at the time of rounding the Cape of Good Hope, or soon after, that scurvy generally appears, and those whose health has been weakened previously by other causes are almost invariably its first victims. Men whose constitution is tainted with syphilis, or who have suffered from climatic disease, and have perhaps been shipped direct

from an hospital in some tropical seaport, begin to complain of muscular pains and extreme lassitude. The symptoms are often so obscure as to give rise to the suspicion of malingering. Mischief, too, is sometimes done through the excusable error of the master in treating them as cases of venereal, rheumatism, and other diseases. Instances are not rare in which even the more characteristic signs of sponginess of the gums and mouth, and ecchymosis on the extremities, have been overlooked, and the patients, as well as their shipmates, have been in ignorance of the true nature of the disease until their arrival in England.

There is often no emaciation, nor much outward indication of illness ; yet exhaustion is so great that death sometimes occurs suddenly after slight exertion, or they have to be hoisted on board the *Dreadnought* in the last degree of prostration. There, recovery is generally speedy and satisfactory ; yet there is reason to believe that in many cases, as pointed out by Dr. Barnes, irretrievable damage is inflicted on the constitution, and the barrier is weakened against the invasion of other diseases. Phthisis and syphilis, in particular, appear to acquire increased intensity. The latter, indeed, is as much the bane of the merchant navy as scurvy is. Both cachexies react injuriously on each other. Conjoined with intemperance, hardship, and fatigue, they shorten the lives of individuals to an incalculable degree, and, by the disgust and terror they inspire, threaten almost to extinguish a most useful class of men, and to paralyze an arm which has been, both in peace and war, of the utmost value to the country.

In all the vessels inspected the outbreak of scurvy could be directly traced to privation of vegetable food, or of its recognised substitute, good lime or lemon juice. In a few instances the provisions were deficient in quality and quantity, but as a rule they were unexceptionable. The same may be said of the men's quarters, and other hygienic conditions of the ships. Nearly all the men examined cheerfully acknowledged that, when sick, they had been treated with humanity and attention. But in no case was a wholesome *mixed* dietary used, such as is enjoined by law for our emigrants, convicts, soldiers, and seamen of the Royal Navy. The lime-juice, which is an undoubted preventive, during the time of an ordinary voyage, if issued as the Act requires, was invariably found to be either of bad quality, or served out irregularly, or neglected to be taken by the men themselves. In some cases it was not the juice of limes, but a spurious compound—a solution of citric acid flavoured with oil of lemons, which is much affected by shipowners and masters as less costly and less liable to spoil than the genuine juice. Experience has amply shown that, however plausible on chemical grounds this may be, it is, like all other medicated substitutes, comparatively valueless as an anti-scorbutic. More often the lime-juice, originally good, had deteriorated from being kept carelessly in a large cask, and become either unfit for use or had lost by decomposition all its prophylactic virtues. An unaccountable prejudice prevents many merchant seamen from using even good lime-juice, and its use seems in all cases to be left too much to their own discretion. Their officers rarely take it, yet seldom suffer from scurvy, although their food in the smaller ships, which seldom carry passengers, differs but little from that of the crew. Yet that slight difference is essential ; a few preserves, or

vegetables that will keep, and the occasional use of beer or wine, prove sufficient to insure their safety. Although the young and feeble are the first affected in scurvy ships, the disease in time spreads in some degree to all, and by the time they arrive in the cold tempestuous latitudes of the Bay of Biscay and the English Channel, the crew is often so thinned by disease as to endanger the safety of the vessel. Even when the malady has distinctly shown itself, the island of St. Helena is in many cases passed by without touching for the supplies which would at once cure it and prevent its extension. Surely this is culpable neglect. It is sometimes explained by the stringent orders of the owners prohibiting all delay. Many scorbutic invalids are left, however, at St. Helena. Mr. Leach informs us that about thirty per annum are admitted into the island hospital, whose average treatment lasts thirty-five days for each, to say nothing of the very numerous cases of out-patients, in whom the symptoms are milder.

Details were entered into of mismanagement and improvidence that led directly to the disease in the vessels inspected. In some instances the masters seemed most to blame. In most, however, the chief responsibility rests with the owners, who practise and inculcate an ill-judged parsimony, which is attended, as we have seen, with the most pernicious results. It is well known that many shipowners pay great attention to the health and comfort of their crews, and that scurvy among them is as obsolete and unknown as in her Majesty's ships.

In the interest of a helpless, friendless class of the community, more isolated than any other, and including thousands of foreigners, who are attracted by the high rate of wages to our mercantile marine, it would seem to be the province of the State to interfere, and by more efficient legislation to lessen the evils that now so notoriously exist. Much was done for the seamen by the Act of 1854, but as germane to this matter it is defective in some essential points—1st, in not positively enjoining a *mixed* scheme of diet, of which fresh vegetables, or the best preserved substitutes for them, should form a part;* 2nd, in not insisting that lime-juice, in the form of lemonade, shall be served out *every day* on which fresh vegetables are not actually issued; and 3rd, in not providing that the lime-juice shall be ascertained to be of good quality, and supplied in such a form as to be reliable for at least two years as an anti-scorbutic. Regulations on the 1st and 2nd heads are easy and obvious; that on the 3rd is quite as essential.

Some interesting details were given as to the varieties of lime-juice and its analysis, and samples were shown of the nauæous, worthless stuff that is found under that name in scurvy-stricken ships. Even the best lime-juice should be mixed with 10 per cent. of spirit, and packed in sealed vessels not exceeding *half a gallon*. It should be given even with fresh meat, if, as sometimes happens, no fresh vegetables are procurable; and in the latter part of the homeward voyage the allowance should be increased to loz. per diem. All the substitutes for it now in use—citric acid, salts of potass, &c., should be prohibited. For secur-

* It might also include, with great advantage, an occasional ration of beer or light wine, which are anti-scorbutics of great value.

ing a good article, *ab origine*, various plans have been suggested, among others, certificates from the vendors, warranting its purity and non-liability to decomposition. Respectable dealers receiving a fair price, would not decline a responsibility of this kind, and adulteration would be checked; or the lime-juice might be supplied to ships only from bond, after having been tested there, and mixed with the proper amount of spirit. Perhaps the most efficient check would be an official inspection, not only of lime-juice, but of the other provisions, more especially the anti-scorbutics. The Board of Trade, recognising the importance of such preventive inspection, suggested the appointment of a medical officer for the purpose at nine of the chief ports. Control in such questions is vested in the Local Marine Boards, but, with the exception of London and Hull, the proposal was emphatically negatived by those boards as "unnecessary, impracticable, and interfering injuriously with the business of the port." It is worthy of note that it is from some of those very ports that the most flagrant cases of scurvy ships proceed, and where the adulteration and counterfeiting of lime-juice is most practised.

In a vessel from Sunderland, not long ago, two deaths from scurvy occurred, and four others of the crew were long in the *Dreadnought*, suffering from the cruelest form of that disease. The lime-juice supplied to that ship was a solution of citric acid.

Other instances of the same kind were adduced from Liverpool, Glasgow, and other ports, in which the result was not fatal, but many of the crews endured great misery from scurvy in its most intense form.

Some observations were made on the medical treatment of the sufferers, particulars of which are required by law to be recorded in the official log-book. Where the disease was recognised by the captain the remedies given were judicious, but, in the absence of the necessary articles of food, were of little avail. Great allowance is to be made for those officers, who are often placed in very trying circumstances. They have too frequently reckless and insubordinate crews to govern, and have to do their best amid difficulties which can be realized in no other situation.

It seems desirable that some knowledge of the elements of hygiene should be acquired by young officers of the mercantile marine, and that they should pass a simple examination on the subject of preserving the health of the men they are destined to command. Erroneous ideas that now prevail on dietetics and other matters would thus be corrected. With seamen well cared for and contented, discipline and right feeling could be more easily maintained. Signs of decay in the mercantile marine are said to be so evident that its present depressed condition and the causes that have led to it are likely to engage the immediate attention of Parliament.

Much of the physical suffering of seamen may, doubtless, be alleviated if their diet, which is much the same kind as it was a century ago, were improved as indicated above. Various other points of hygiene in the merchant navy were discussed at length, and allusion was made to the necessity for an extension of the recent Contagious Diseases Act to our great commercial seaports, where it is even more imperatively called for than in garrisons or naval stations.

Much benefit also was anticipated to the men, the masters, and owners of the small-class ships, which, as a rule, have no medical officers, if the crew were systematically submitted to a medical examination at the time of being entered for the voyage.

(B.) CONCERNING ACUTE DISEASES.

ART. 13.—*On the Practical Value of Accurate Daily Observations of the Temperature of the Body in Acute Disease.*

By Dr. THOMAS ARMETIRDING COMPTON.

(*Dublin Quarterly Journal of Medicine*, August, 1866.)

The general conclusions which Dr. Compton has come to have been arrived at after a careful study of some 125 cases taken by himself during the last two years, at St. Bartholomew's Hospital, and also of some seventy-five other cases taken in the same hospital, during the same period, by Dr. Warter.

The total number of cases in which the temperature and general symptoms have been watched and recorded daily throughout their course, amounts to 200, of which sixty are typhus, thirty typhoid, twenty pneumonia, fifteen scarlet fever, and the remaining seventy-five comprise cases of febricula, acute rheumatism, erysipelas, cholera, acute tuberculosis, &c. The total number of observations in these cases, and in others in which only one temperature has been recorded by Dr. Warter or Dr. Compton, probably exceeds 5,000. Dr. Compton states what he considers to be approximately the average normal temperature of an axilla in a healthy adult. A temperature of $98^{\circ} \cdot 4$ Fah. is the point generally settled upon by the majority of authorities on the subject; but this, Dr. Compton believes to be too high, as although he has not at present taken a sufficiently large number to decide the question to his own satisfaction, yet he can state that he has very rarely found such a temperature present in a healthy adult under normal conditions. "I have," he says, "every reason to think such a temperature to be nearly up to the maximum, consistent with health, and to be only met with occasionally, just as one comes across, now and then, a healthy adult with a temperature below 96° Fah. I consider the healthy range to be somewhere between $95^{\circ} \cdot 5$ and $98^{\circ} \cdot 5$ Fah., the most common temperature met with, being probably $96^{\circ} \cdot 4$ Fah., *i.e.*, one degree less than the temperature hitherto most generally received as the normal one."

Dr. Compton seeks to establish the following propositions from his observations:—

1st. That a continued daily temperature of 99° Fah., and upwards, indicates an unhealthy condition, and occurs in every case of acute disease.

2nd. That any one observation of a very high temperature (such as 105° Fah.), in any case in which the general symptoms do not appear of any particular severity, should lead to a very attentive re-examination,

and suggest a very careful watching, especially if occurring in a non-diagnosed case; such a temperature being present only in severe forms of any disease.

3rd. That the thermometer is of great use, as a means of diagnosis in those cases, which frequently present themselves, of general *malaise*, often accompanied by a history of rigors, loss of sleep, &c.; such symptoms being due either to the commencement of one of the specific fevers, or merely to some gastric or uterine disturbance of a temporary character.

4th. That the temperature in every disease has a tendency to run a peculiar course, and has a certain range of altitude, a knowledge of which course and range is of great value as an assistance to us in diagnosis and prognosis.

5th. From the last proposition it follows, that the same altitude of the thermometer attained at one period of any disease is not of the same importance as the same height reached at another time in the same disease.

Thus, in typhoid fever, a temperature which has been rising for two or three days, reaches perhaps 104° Fah. between the seventh and fourteenth days, without causing any anxiety; whereas should the same phenomenon occur about the twenty-eighth day, a fatal termination may probably be expected.

And again, the actual altitude attained on a certain day in one disease is not of the same importance to our prognosis as the same height reached on the same day in another disease. Thus, a temperature of 104° Fah. in erysipelas is very common during the first week, and need not give rise to any alarm; but should such occur at the same date in acute rheumatism Dr. Compton would consider it of much more importance.

6th. That although, in all diseases, a high range of temperature generally indicates a severe case, with a slow convalescence, and a low range usually occurs in a mild case, and is followed by a rapid convalescence; yet there is no actual temperature in any disease which necessarily foretells a fatal termination.

7th. That in the majority of cases a rise of temperature is contemporary with a rise of pulse, although such is often not a proportional one, and may not take place at all unless the alteration in temperature be as much as $1\frac{1}{2}^{\circ}$ or 2° Fah.

8th. That where the temperature and pulse together do not coincide with the general symptoms, the two former may be generally relied on as to the actual state.

9th. That where the temperature and general symptoms agree together, but do not coincide with the state of the pulse, the two former may generally be relied on as to the actual state.

10th. That in those cases in which the pulse and general symptoms remain the same, a moderate fall of temperature on one occasion is not to be relied on; but should such a fall continue in a moderate and gradual manner, for some days, and at such a period when a fall was to have been expected, the temperature may then be depended upon. Severe cases of typhus, towards their close, often give examples of this sort.

11th. That in those cases in which the pulse and general symptoms continue the same, being the one frequent and the other severe, a continuous rise of temperature for some days, occurring at a period of disease at which some improvement might generally be expected, is usually the precursor of a fatal termination.

12th. That although it is possible that the state of the temperature alone in acute disease may, perhaps, hereafter prove to be the one safest symptom to rely upon if taken by itself (and I believe it is at present, at least, equal to the state of the pulse, and of greater value than this certainly, if only its frequency be taken into account), yet the temperature must be considered merely as an aid, and all other symptoms must be carefully examined into, as it is on comparison with these that its greatest value is always to be found.

ART. 14.—*On the Utility of Hyposulphites in the Treatment of Diphtheria.*

By Dr. HAYDEN, Physician to the Mater Misericordia Hospital, Dublin.

(*Dublin Quarterly Journal of Medicine*, August, 1866.)

Dr. Hayden relates a series of cases of diphtheria to illustrate the remedial action of the hyposulphites, and in the hope that others may give these agents a more extended trial in the treatment of diphtheria. He discusses the general pathology of the disease only so far as it seems necessary in order to justify what may be called specific treatment. Dr. Jenner declares that "diphtheria is a general disease—runs a quick and definite course, and has a specific cause." In this opinion Dr. Hayden entirely concurs; and, indeed, he says, it will be difficult to controvert it, if due weight be given to the epidemic character of diphtheria—the great debility which accompanies, and frequently succeeds it—the frequent occurrence of albuminuria, and of specific exudation upon abraded surfaces, and lastly, the remarkable lesions of motion and sensation which are occasionally met with amongst the sequelæ of the disease.

"If diphtheria, then," Dr. Hayden continues, "be a general disease, manifesting itself by constitutional as well as by local symptoms, it follows that general treatment is indicated; and further, if we admit that it is a specific disease, we are, as it seems to me, warranted in searching for a specific remedy. The remarkable experiments of Dr. Polli, of Milan, who has succeeded in arresting putrefactive fermentation by means of the hyposulphites of the earths and alkalis, encourage the hope that zymotic or catalytic diseases may be controlled by the same agents administered medicinally. It is true that both Bretonneau and Trousseau declare they have no confidence in the general treatment of diphtheria; but medicine is a progressive science, and we should not regard as final the adjudication of even the most eminent of our brethren upon a question still open to investigation.

"In *The Dublin Quarterly Journal of Medicine* for August, 1864, Dr. De Ricci has published some cases of septicemia successfully treated with bisulphite of soda. Encouraged by these results, as well as by several cases of diphtheria which recovered under the hyposulphite treatment, reported from time to time in the journals, I determined to give these agents a trial; and although some of my cases were examples of the milder form of the disease, or the 'common membranous angina' of Bretonneau, others were sufficiently serious to warrant an unfavourable prognosis. Death occurred only in one instance amongst the eight cases; in that instance alone the hyposulphite was not given; the patient was seventy years of age, and his case was complicated with congestion of both lungs.

"I do not mean to assert that in the hyposulphites we have got an agent capable of neutralizing or decomposing the toxic principle of diphtheria, whatever that may be; but of their curative properties in this disease I think we have sufficient evidence to warrant us in giving them a more extended trial. The only inconvenience which I have experienced in the use of the hyposulphite of soda was the occasional occurrence of diarrhœa, but this was readily corrected by the addition of a little syrup of poppies, or other mild astringent."

ART. 15.—*On the Treatment of Rheumatic Fever.*

By J. BIRKBECK NEVINS, M.D. Lond., Liverpool.

(*British Medical Journal*, September 8, 1866.)

Dr. Nevins, in reference to a growing tendency to consider that it is a matter of indifference whether anything at all is done in rheumatic fever, except to keep the patient quiet in bed, and supply moderate nourishment, submits a plan of treatment which he has long practised, and which it appears to him has led to the following results:—1. Speedy relief of the patient's most urgent symptoms; 2. Diminution of the general duration of the case; and 3. Restoration of strength, with less tendency to heart-complications or relapses than usual.

The remedies to which he attaches importance are:—

1. The vapour-bath, and subsequent cold douche; and
2. The combined use of quinine and iodine.

In a case related, the bath was given in bed, for the patient could neither turn in bed nor move his limbs; "and," writes Dr. Nevins, "it will generally be necessary to give it in bed, in the first instance, in any case deserving the name of rheumatic fever; and it is so easily administered, that no difficulty can arise to prevent its employment in every case.

"Two large pieces of coarse flannel (common scouring cloths answer the purpose admirably)," he continues, "are to be soaked in common vinegar;* about a pint being necessary for each cloth. Two common

* For many years I soaked the flannels in simple water; but the vinegar

bricks are then to be heated nearly red-hot in the fire, folded up in these flannels, and placed on two plates. The patient being stripped, one plate is to be put a little distance from one knee and the other a little distance from the opposite shoulder, and the patient is to be covered over with the bed-clothes. In a few minutes, he is surrounded by a most refreshing steam-bath, which produces a warm, agreeable perspiration, that may be kept up for twenty minutes or longer, if the bricks retain their heat sufficiently.

"As soon as it is decided to remove them, the patient, still in bed, is to be very rapidly mopped all over with towels wrung out of cold water, then immediately wiped dry with dry towels, supplied with a warm shirt or flannel garment, and covered with a fresh dry sheet, &c., or with blankets alone, as may be most agreeable to him.

"The effects of this bath are a speedy relief of the acute pain, and frequently easy sleep for a time; an abatement of the offensive and distressing acid sweats; and a general state of greater comfort.

"The cold water application immediately on the removal of the hot vapour is very important; as it prevents the continuance of an enfeebling perspiration after the hot bath.

"The manner of removing the patient's bed garment is a point of importance in cases of such painful helplessness as rheumatic fever; and it is accomplished without pain to the patient or difficulty to the nurse by an extremely simple contrivance. The clothes must be torn down the back from top to bottom; and when this is done they can be removed and replaced as easily as a child's pinafore, without even lifting a limb of the patient or disturbing him in bed. By this means, fresh, clean, dry clothing can be applied without difficulty one or twice a day, according to the amount of sweating; and the sufferer is relieved from the discomfort of his damp, offensive garments.

"This bath may be repeated twice a week; and during seventeen years that I have been in the habit of adopting it, I have scarcely ever had to use it a third time in bed; the patient, after the second bath, being almost invariably able to sit up and have the third in a chair.

"When he is able to sit up a steam-bath can be given with great ease by putting a bucket of boiling water under a chair, the seat of which is sufficiently protected to prevent the patient from being scalded, whilst he is sitting upon it surrounded by blankets; and, by putting a red-hot brick into the water in the course of ten minutes, the steam is kept up, as by this time it generally begins to abate from the original boiling water.

"A jug of cold water may be poured over the patient when the blankets are removed, or he may be wiped by cold wet towels, as is most agreeable to his own fears or feelings, and he must then be clothed and sit up for a few hours.

"The second part of the treatment upon which stress is laid, is the combination of moderate—*i.e.*, two-grain doses of quinine with five-grain doses of iodide of potassium from the first. The theoretical

is so much more fragrant and agreeable to the patient, that I have always used it for the last few years.

grounds on which quinine was first proposed have been already mentioned; and the general experience of the profession will suggest the explanation of the probable benefit to be looked for from the addition of the iodine."

ART. 16.—*On the Nature of the Poison of Contagious Diseases.*

By Dr. LIONEL S. BEALE, F.R.S.

(*Medical Times and Gazette*, September 22, 1866.)

In his report to the Royal Commission on the Cattle Plague, published in detail in the third report of the Commissioners, Dr. Beale has—

(1.) Advanced facts and arguments which seem to him opposed to the view that the contagious matter consists either of insects, of animalcules, or any kind of vegetable organism.

(2.) He thinks that it consists of living matter formed in the organism of man or animals—the particles being exceedingly minute, and capable of retaining their vitality for a long time, and under various conditions, although separated from the body.

(3.) That these living particles bear somewhat the same relation to the germinal matter of normal cells that pus corpuscles or cancer cells do, and therefore that the contagious germs have been derived by continuous descent from the normal germinal matter of the organism. They may have descended from a lymph or white blood corpuscle, or from the germinal matter of an epithelial cell.

(4.) If this be so, the living contagious particle is not, Dr. Beale holds, of the nature of a *parasite*, nor can it be regarded zoologically as a species, nor has it originated in the external world and grafted itself upon man, but it has originated in his organism, and is, indeed, degraded living matter descended from what was once normal living matter of the body itself.

ART. 17.—*On Mixed Types of Fever.*

By Dr. HENRY KENNEDY, Physician to the Cork-street Hospital, Dublin.

(*London Medical Press and Circular*, June 20, 1866.)

Dr. Kennedy discusses the subject of mixed types of fever, in relation to the question of the identity or non-identity of the typhus and typhoid poisons. He believes that the typhus poison is capable of engendering the type of fever known as typhoid or enteric, and that this particular type must be due to some other cause rather than a specific poison. On the other hand, he holds that the two types can, in the great majority of instances, be distinguished the one from the other. He records a series of fourteen cases of "mixed types" of fever, on which he offers the following observations:—

“Such is the series of cases which I wish to bring under notice, When added to those already given in the former papers—and, did time permit, I could have given other similar cases—they appear to me to afford the strongest proof the question is capable of eliciting, that we must consider the two types of fever known as typhus and enteric as the result of but one poison. If this be not the correct view to take of the matter, I confess myself quite unable to explain the cases of the mixed types detailed in this paper; for it must have been observed, as each was given, how the symptoms of each type of fever were mixed up together. As there is not time, however, to go over each symptom in detail, I shall notice but one, on which most, if not all, who hold different views from my own, seemed to have placed the greatest weight of their argument. I mean the spots said to be characteristic of enteric fever. On this point, I think I may say with certainty that these lenticular red spots, and few in number, have not the value which has been given them; for I have seen them now in many instances, and some have been given where, while they existed, there was not another symptom of the ileum being engaged—at least I could make out no evidence of such a lesion, though looking specially for it. Here, then, were cases where the particular spots existed, but not the lesion of which they are said to be diagnostic. But, further still, I have given cases where, with the enteric spots, there was also a typhus rash. As bearing on this particular point, I would just recall the case of the man Develin, where the enteric spots first appeared, then the typhus rash, and as this latter disappeared the enteric spots were again visible. If this be not a case in point, I know not what is; and I shall be glad to hear some explanation of this from any gentleman who differs from me. As regards the spots of typhus fever generally, I have got an impression that a good deal of misconception exists. I have heard some speak of the bright and the dark spots, as if there were a difference between them. On this point I can state with certainty that it is very common to see the two on the same individual, and at the same time. This may be seen on the body itself, but it is more common to have them dark on the body and a bright red on the arms. Again, the spots of enteric fever are described as recurring again and again, and this is quite true. But it does not seem to be so generally known that the same may be seen in typhus, for I have witnessed cases where a distinct second crop of eruption appeared; nor is the observation original, as I have read of it in one of the olden authors, though I cannot at this moment give his name. So also of the statement that petechiæ are never seen on the face. This is positively incorrect, as I have noted several cases where they were quite distinct. But these points are only mentioned here as bearing indirectly on the point under discussion. Still I think they are enough to show that any positive statements about the rash in fever must be received with caution, as the variety is truly very great.”

ART. 18.—*A Case in which Typhoid Fever and Measles were coincident.*

By W. B. KESTEVEN, F.R.C.S.

(*The Lancet*, June 9, 1866.)

On the 14th, 15th, and 16th of last December, a girl, aged fourteen years, was ill with measles. A few days afterwards another girl, aged eleven years, was indisposed, and suffering from symptoms of fever of the enteric type. On or about the 30th of December, a few of the rose-coloured spots characteristic of typhoid fever made their appearance, and increased in number, but did not exceed thirty or forty over the whole of the trunk. On the 8th of January, two younger children presented the ordinary symptoms and eruption of measles, and passed through the disease in the usual course.

On the 8th of January Dr. Kesteven was considerably puzzled by finding his typhoid patient exhibit the general symptoms and special rash of measles in addition to that of the enteric fever. He began to doubt the correctness of his diagnosis as to either the one or the other of the two diseases, and was, therefore, only too glad to avail himself of the superior judgment and greater experience of Dr. Jenner, who, after careful investigation into the history and condition of the patient, confirmed his opinion that he had here a case in which these two eruptive fevers were coincident.

ART. 19.—*A Suggestion for the Analysis of Infectious Essences.*

By ARTHUR LEARED, M.D., M.R.I.A., Senior Physician to the Great Northern Hospital.

(*The Lancet*, August 18, 1866.)

“However opinions differ in particular instances,” writes Dr. Leared, “it is undeniable that certain diseases are propagated by infection. Effluvia capable of producing disease, such as malarious poison, probably products of decomposing organic matters, are given off by the earth. In neither instance has any one yet demonstrated the existence of the essences concerned, because they elude the senses, and no means of analysing them has been hitherto discovered. A conviction of their existence is only arrived at from observation of their effects. Nevertheless the demonstration would in many respects be of great importance. Thus, in the case of cholera, the discovery of such an essence would settle the question of its infectious nature—still a debatable point. And supposing any infectious emanation to have been demonstrated in any given disease, or the existence of malarious poison in any given place, it could then be ascertained with scientific accuracy what amount of atmospheric dilution would be necessary to render it

innocuous. It is known that, within certain limits, fever cases may be introduced almost with impunity in a ward amongst other cases. But were we possessed of an instrument by means of which the safe point of atmospheric dilution could be determined, as the temperature is ascertained by the thermometer, it would surely be an important gain. Now, unless my anticipations deceive me, we do possess such an instrument, although its adaptation to the present purpose is entirely novel.

"No one who has witnessed the beautiful experiments of Prof. Tyndall on the radiation of heat could fail to be struck by the unexpected nature of the results, as well as by their undeviating uniformity under given conditions. He has demonstrated that the power of intercepting the waves of heat possessed by the molecules of various gases and odorous emanations differs vastly in degree. Thus, taking the quantity intercepted by atmospheric air as unity, the relative absorption of nitrous oxide is 1860, and of sulphurous acid 6480; of the odour of lavender 60, and of aniseed 372. Professor Tyndall has even shown that radiant heat may be 'employed as a means of determining practically the amount of carbonic acid expired from the lungs.' This mode of quantitative analysis is evidently capable of much extension.

"Now if such attenuated matter as that concerned in the odours given off from certain bodies is so capable of demonstration by this new method, it is almost certain that the emanations which are concerned in propagating disease will also yield appreciable results.

"The possible great importance of this new mode of research induces me to bring the matter before the profession, even in the form of a suggestion. The opportunity of examining air obtained from close contact with the body, and also of the breath in cholera and in the cattle plague, is now presented, and should not be allowed to pass by. A description of the ingenious instrument of Professor Tyndall would take up too much space. It will suffice to say that, although delicate and requiring skilful treatment, its management presents no insurmountable difficulties; and I have it from Professor Tyndall himself that my proposed investigations promise well."

ART. 20.—*On the Phenomena of Asphyxia in Cholera.*

By M. JULES BESNIER.

(*Archives Générales de Médecine*, September, 1866.)

M. Besnier has subjected the question of the production of asphyxia in cholera to a careful examination during the late epidemic in Paris. Of the pathological condition of the pulmonary organs he states, as the result of his observations, that the lungs had shrunk but slightly, sometimes not at all. The superior lobes were notably emphysematous, little crepitant, and often contained but a small amount of blood. But the inferior lobes always presented upon their exterior surface a more or less intense reddish discolouration, sometimes violet. When cut, a considerable quantity of black fluid blood ran out, which reddened

slowly on exposure to the air. In some cases this congestion was so great as to have produced an apoplexy by infiltration, and even to have led to hepatization of the free borders of these organs. This condition of the lungs denotes, says M. Besnier, more or less intense congestion, and it differs especially from that which is observed in ordinary cases.

The *trachea* and *bronchi*, with their divisions, exhibit lesions not less remarkable.

Throughout the entire extent of these organs M. Besnier has found upon the bronchial mucous membrane a deposit of a reddish, viscous matter, forming a kind of jelly on the surface. Sometimes half transparent and of a carrot-red, at other times it was slightly yellowish, and at some points greyish. The quantity was variable: it often formed a layer sufficiently thick to block markedly the bronchial tubes. It was with difficulty detached by a very strong current of water. This matter was found more or less uniformly spread throughout the ramifications of the bronchi, but it seemed to be more abundant in the remoter divisions of the tubes, doubtless from their smaller calibre. In many cases, before opening the bronchia, foreseeing, from previous autopsies and the symptoms observed, the presence of this deposit upon the mucous surface, M. Besnier has insufflated the lungs. He has thus caused an augmentation of the volume of the organs, but rarely to the extent obtained by insufflation where the deposit was absent. A microscopic examination of this viscous matter, made by M. V. Limoine (de Reims), gave the following results. There was a considerable quantity of cells, some narrow and elongated, others larger and irregular, and both kinds provided with vibratile cilia at one of their extremities. Each cell had a distinct nucleus in the midst of minute granules, and the nucleus was sometimes voluminous and filled with granules. Mingled with the preceding cells were other cells of an irregular or rounded form, without vibratile cilia, and in which the nucleus was sometimes well defined, at others replaced by granules. Lastly, between these different cells were found numerous granules analogous to those which filled the cells and the nuclei. The cells were evidently the epithelial cells of the bronchial mucous membrane, which, rare in a normal state, had become very abundant and voluminous. Some were in process of formation; others were completely developed; others, again, were more or less disorganized, the vibratile cilia having been cast off, and the granular contents discharged by the rupture of the walls; but they still, for the most part, maintained the characters assigned to pus.

Below the epithelial deposit described, the bronchial mucous membrane presented an intense uniform redness; in some cases here and there it was slightly softened. Upon the red surface, the longitudinal and transverse striæ of the fibrous tissue were clearly distinguished by their greyish colour. M. Besnier has not observed in any case the glandulous eruption mentioned by some authors.

The lesions of the pulmonary organs thus described present some variations. They were very marked when the asphyxia had been *rapid*; but when the asphyxia had been *slow* and *progressive*, the pulmonary congestion was slight, and the epithelial deposit appeared to M. Besnier to be more abundant, and to be accompanied by an emphysematous condition of the lungs, more marked and more extensive than in the rapid cases.

From the lesions observed, M. Besnier contends that the asphyxia of cholera arises from the obstacle opposed to the entry of the air into the pulmonary vesicles ; and he sets aside, as insufficient, the hypotheses that the asphyxia is due to a special alteration of the blood, which prevents its arterialization, or to the non-penetration of this liquid into the pulmonary capillaries. He supports his opinion by many cogent arguments, especially insisting upon the fact that the state of the mucous membrane he describes is peculiar, so far as his observation of the morbid pathology of cholera extends, to those cases in which asphyxia is a predominant symptom. This phase of the disease is not to be confounded with the asphyxia which occurs at the termination of the algide period. The latter form of asphyxia is but of secondary importance as compared with other morbid phenomena, and is of short duration.

ART. 21.—*On the Detachment of the Intestinal Epithelium in Cholera.*

By Dr. LIONEL S. BEALE, F.R.S.

(*Medical Times and Gazette*, August 18, 1866.)

On this question Dr. Beale writes :—

“In almost all the cases of cholera I have yet examined there is evidence of chronic structural change in the *tissues* of the intestines, and I think we shall be led to conclude that in most of the cholera victims important morbid alterations have been going on for months, and in some instances for years, before death. In some cases it is probable that, had the individual escaped cholera, he must have succumbed to some other malady within a short period of time. The columnar epithelial cells often exhibit evidence of chronic change ; they seem to be stunted, and in many instances the nuclei are much smaller than in health. In the intervals between their attached extremities one fails to find those smaller and younger cells which in the healthy state gradually grow up to take the place of those cells which are removed and give origin to new cells, which in their turn become developed. So also it is to be observed that the masses of germinal matter so numerous near the surface of the healthy villus are almost absent in many of these cases of cholera. And there are other and very striking changes in the structure of the affected villi which I shall describe fully in other communications.

“Cholera seems to be so constantly associated with the removal of columnar epithelium from the villi, that we have been led to look upon this as one of the *essential* phenomena of the disease. Although there may be no actual diarrhœa, this epithelium is found in quantity in the intestine after death. It may be said that this removal of epithelium occurs immediately after or only just before death, but the great number of columnar epithelial cells and entire sheaths of the villi so frequently found in the rice-water evacuation, giving to it its peculiar character, proves that such a notion is not tenable. Can cholera exist

without the villi being denuded of their epithelium?—is a question which, as far as I know, has not yet been answered, but which must be answered before we can form a correct notion of the nature of this most wonderful disease. I do not think there is any other morbid condition in which this striking change is observed—at any rate, to the extent or with the frequency it is met with in cholera. It seems, however, likely, that where those changes in the blood occur very quickly indeed, so as to cause death by sudden stagnation of the blood in the capillaries of important organs, there might not be time even for the removal of the epithelium from the villi, just as we may have death from small-pox or scarlatina without any eruption.

“With reference to the denudation of the villi, it must be borne in mind that the throwing off of epithelium is not confined to the villi of the intestine, or to that of the intestinal mucous membrane generally. The process affects the mucous membrane of the gall-bladder and larger gall-ducts; that of the bladder, ureters, and pelvis of the kidneys, as well as that of the Fallopian tubes, uterus, and vagina. In short, there seems a tendency to the removal of epithelium from the surface of all the soft, moist, mucous membranes; not, it must be remembered, of the epithelium which is specially concerned in *elimination*, but rather of that which lines the *ducts* of glands and cavities which may be included in the category of the ductal portion of the different secreting glands.

“On the other hand, there is no evidence of the increased formation or more rapid-removal of the secreting epithelium in the various glandular organs. The follicles of the mucous membrane of the stomach and intestine, those of the salivary glands and pancreas, the tubes of the liver, kidney, and other glands, still retain their epithelium; nor have I been able to demonstrate in these varieties of glandular epithelium any appearances peculiar to cholera. Indeed, so far as I have yet been able to observe, it would be extremely difficult to distinguish many secreting cells taken from the bodies of cholera victims from perfectly healthy cells.”

ART. 22.—*Instructions of the College of Physicians for the Treatment of Cholera.*

The Lords of Her Majesty's Privy Council having by their medical officer, Mr. Simon, addressed a letter to the College of Physicians relating to the expediency of issuing instructions to captains of merchant vessels “how they should act when proper medical attendance cannot be procured, so as to provide for the health of their crews against attacks of cholera,” the following is the substance of the reply* forwarded by the College:—

“Their Lordships request to be informed ‘whether, in the opinion of the College, any, and if so, what suggestions might be issued as repre-

* The reply adopted by the College was drawn up by a committee consisting of the following gentlemen:—Dr. Gull, Dr. George Johnson, Dr. Jenner, Dr. Milroy, Dr. Parkes, and Dr. J. Burdon Sanderson.

senting the present state of medical knowledge and experience with regard to the drugs which should be given, or other treatment which should be adopted, in attacks of cholera, and especially in the beginning of the disease, when proper medical attendance cannot be procured.' Their Lordships, at the same time, submit to the College a copy of the instructions issued on previous occasions.

"With reference to that part of the instructions on which their Lordships particularly request the opinion of the College—viz., that which relates (1) to the necessity of avoiding purgative medicines during the prevalence of cholera, and (2) the measures to be adopted when cholera appears on board ship—the Committee think—

"1. That when opening medicine is required the mildest should be selected, as castor-oil or rhubarb. Glauber's salts and Epsom salts are dangerous. The common belief that prolonged costiveness should not be interfered with during the prevalence of cholera is erroneous.

"2. That the master should ascertain by inquiry, morning and evening, whether any of the crew are labouring under diarrhœa, and if so the following recommendations are subjoined for his guidance:—

"3. That if a man be attacked with diarrhœa he should, whenever it is possible, be sent to bed and kept warm, and some aromatic and astringent medicine, containing a small quantity of opium, should be given to him at once, and should be repeated every hour or two, according to the severity of the purging.

"It is suggested that ten grains of the aromatic powder of chalk and opium (of the *British Pharmacopœia*) should be so given in half a glass of peppermint water or weak brandy-and-water. Should this medicine not be at hand, five measured drops of laudanum may be substituted for each dose of the powder.

"Large doses of opium or of ardent spirits should be avoided.

"If the diarrhœa should result from bad or obviously indigestible food, or if the discharges are unnaturally offensive and attended with griping pain, it would be desirable to give a dose of either of the gentle laxatives above named before administering the opiates.

"The diet should consist mainly of beef-tea or broth, gruel, or rice.

"If the discharges become colourless and watery (the purging being of the kind commonly called 'rice-water purging'), and be accompanied with vomiting and coldness, the opiates should no longer be persisted in, and spirituous liquors should be avoided. The patient should be strictly kept in the recumbent position, he should be allowed to drink water freely, and should be abundantly supplied with fresh air. Warm applications should be used to the feet and legs, and a mustard poultice should be applied to the pit of the stomach. Cramps may be treated by rubbing the affected parts with the warm hand.

"In all cases, medical advice, when obtainable, should be obtained as soon as possible."

ART. 23.—*Rules for the treatment of Epidemic Diarrhœa and Cholera.*

By GEORGE JOHNSON, M.D., F.R.C.P., Physician to King's College Hospital ; Professor of Medicine in King's College, &c.

(*British Medical Journal*, July 21, 1866.)

Dr. G. Johnson sums up his matured views of the treatment of epidemic diarrhœa and cholera as follows :—

“ It may be stated as a general proposition, that the immediate cause of diarrhœa or looseness of the bowels is the presence of offending materials in the alimentary canal. These offending materials are of various kinds in different classes of cases. In one case, unwholesome and undigested food is the exciting cause of the purging ; in another case, a large and unnatural accumulation of the fæculent contents of the bowel ; while, in another class of cases, offending materials are poured from the blood into the bowel, in consequence of the action of a morbid poison upon some of the ingredients of the blood. To this last class of cases belongs what is called *choleraic diarrhœa*.

“ The most rational theory of choleraic diarrhœa is, that a morbid poison enters the blood either with the air through the lungs, or with the food and drink through the alimentary canal ; and that this poison excites certain changes in the blood, in consequence of which some blood-materials are spoiled, and thus rendered not only useless, but noxious. These morbidly-changed blood-materials are then discharged from the blood-vessels through the mucous membrane of the stomach and bowels, and are ultimately ejected by vomiting and purging.

“ Various as are the remote and primary causes of diarrhœa, this one condition is common to all classes of cases, viz., that the contents of the bowel are unnatural and offensive. These offending materials are the immediate cause of the purging ; and they must be expelled from the bowel before the diarrhœa can come to an end.*

“ From the above considerations we deduce one important and guiding rule of treatment, which is this—*not to attempt by opiates, or by other directly repressive means, to arrest a diarrhœa, while there is reason to believe that the bowel contains a considerable amount of morbid and offensive materials.* It is certain that these offending materials must be cast out from the bowel before the diarrhœa can permanently cease. The effect of an opiate at this stage is to prolong the disease, and to increase the risk of mischief from the retention and reabsorption of the morbid contents of the bowel. If the opiate have the effect of retaining within the blood-vessels some of the morbidly-changed blood-constituents, this astringent action will probably be more injurious and even deadly than the retention of morbid secretions within the bowel.

* We need not here take into consideration those cases of diarrhœa which result from ulceration or other local disease of the bowel itself.

“The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by directing the patient to drink copiously any simple diluent liquid—water cold or tepid, toast-water, barley-water, or weak tea; and we may often *accelerate* the recovery by sweeping out the alimentary canal by some safe purgative, and then, if necessary, soothing it by an opiate. Castor-oil, notwithstanding its unpleasant taste, is, on the whole, the safest and the best purgative for this purpose. It has the advantage of being very mild and unirritating, yet withal very quick in its action. A tablespoonful of the oil may be taken, floating on cold water or any other simple liquid which may be preferred by the patient. A mixture of orange-juice or of lemon-juice with water forms an agreeable vehicle for the oil. If the dose be vomited, it should be repeated immediately; and the patient should lie still, and take no more liquid for half an hour, by which time the oil will have passed from the stomach into the bowels. Within an hour or two the oil will usually have acted freely. Then a tablespoonful of brandy may be taken in some thin arrowroot or gruel; and, if there be much feeling of irritation, with a sense of sinking, from five to ten drops of laudanum may be given in cold water. These means will suffice for the speedy cure of most cases of choleraic diarrhœa. If the patient have an insuperable objection to castor-oil, or if the oil cannot be retained on the stomach, ten or fifteen grains of powdered rhubarb, or a tablespoonful of the tincture of rhubarb, or a teaspoonful of Gregory’s powder, may be substituted for the oil.

“If the diarrhœa have continued for some hours, the stools having been copious and liquid; if there be no griping pain in the bowels, no feeling or appearance of distension of the intestines; the abdomen being flaccid and empty, and the tongue clean—we may conclude that the morbid agent has already purged itself away. There will, therefore, be no need for the castor-oil or other laxative, and we may immediately give the brandy in arrowroot, and the laudanum, as before directed. The rule in all cases is, *not to give the opiate until the morbid poison and its products have for the most part escaped; not to close the door until ‘the enemy’ has been expelled.* While there are some cases in which the evacuant dose is not required even at the commencement of the attack, there are many more in which the opiate is unnecessary in the later stage. In some cases of severe and prolonged diarrhœa, it may be necessary to repeat the oil and the laudanum alternately more than once, at intervals of three or four hours. Practical skill and tact are required to discriminate these cases.

“If the diarrhœa be associated with vomiting, this should be encouraged and assisted by copious draughts of tepid water. The vomiting affords relief partly by the stimulus which it gives to the circulation, but mainly by the speedy ejection of morbid secretions.

“Thirst may be allayed by drinking cold water, which may be acidulated by the addition of lemon-juice or a few drops of dilute sulphuric acid. *Care should be taken that the water for drinking is pure.* Organic impurities, such as result from the admixture of sewage, are especially to be dreaded. If the water be of doubtful purity, it should be carefully filtered through sand and charcoal, and then boiled. Impure water is a common exciting cause of cholera.

“While the diarrhœa continues, the diet should consist mainly of rice or arrowroot, gruel or broth.

“In all cases of severe diarrhœa the patient should remain in bed.

“If the purging continue, if the stools become colourless and watery (the purging being of the kind commonly called rice-water purging), and if the surface of the body become cold and blue, the disease is now passing, or has actually passed, into the stage of collapse.

“This state of choleraic collapse results from a peculiar arrest of the flow of blood through the lungs, occasioned by a morbid poison. It is not a condition of mere exhaustion. It is not relieved by the remedies for exhaustion; and it is made worse by opiates and by spirituous stimulants, which must therefore be avoided. The patient should be strictly kept in the recumbent position; he should be allowed to drink pure water freely; and should be abundantly supplied with fresh air. Hot flannels or bottles, or bags of sand, should be applied to the feet and legs.

“Cramps may be relieved by rubbing the affected parts with the warm hand.

“Hot baths, whether of water or of air, have been found to be, on the whole, more distressing and exhausting than beneficial.

“Five grains of sesquicarbonate of ammonia, or a teaspoonful of spirit of sal volatile, may be given in an ounce of camphor mixture every two or three hours as a diffusible stimulant.

“The discharges from the bowels, and the condition of the abdomen, should be carefully observed. The discharges always continue, more or less, during the stage of collapse and until reaction has set in. One of the earliest and surest signs of reaction is the reappearance of bile in the vomited matters and in the stools. When vomiting and purging entirely cease during the stage of collapse, the disease is nearly always fatal.

“One of the main objects of treatment during this stage is to facilitate the escape of the morbid secretions from the alimentary canal. This may be done partly by the copious use of diluent drinks, and partly by an occasional dose of castor-oil. If we carefully observe the condition of a patient in collapse, we shall often find that the intestines are more or less distended with liquid, and this, too, while perhaps there is general torpor and little or no effort at expulsion. Again, it has often been found that, although there has been copious watery purging during life, the small intestines contain after death a large amount of a peculiar viscid dirty white material, having a very offensive odour. An occasional dose of castor-oil—a tablespoonful every three or four hours during the stage of collapse—may be useful in removing both these conditions; namely, over-distension of the bowel by liquid, and accumulation and retention of offensive viscid semi-solid secretions.

“The object and the effect of this treatment are not to increase the amount of liquid which is poured from the blood into the stomach and bowels, but simply to assist and to quicken the expulsion of the morbid secretions from the alimentary canal.

“After reaction has occurred, an occasional laxative dose is required—about once in the twenty-four hours during the first two or three days.

“It is worse than useless to attempt to *feed* a patient during collapse. The secretions of the stomach are utterly deranged; and the

power of digestion is suspended. The mildest nourishment administered at this time only adds to the feeling of oppression and general distress, from which the act of vomiting often gives immediate relief.

"After reaction has occurred, and when the normal secretions are restored, the mildest nourishment should be given frequently, but in small quantities—such as milk, gruel, or rice, or arrowroot with a small quantity of brandy, soup, or beef-tea, or chicken-broth. After an attack of cholera, the stomach is sometimes long in recovering its tone and the power to digest solid food. When this is the case, a grain of quinine, with ten or fifteen drops of dilute hydrochloric or sulphuric acid and an equal quantity of chloric ether, may be taken with each meal. The same combination, too, often relieves that distressing sense of uneasiness, with flatulence in the stomach and bowels, experienced by many persons who are not otherwise ill during an epidemic of cholera.

"*Venesection* has often afforded great relief during the stage of collapse. The symptom which appears especially to call for this remedy is rapid breathing, with a feeling of impending suffocation. When, with these symptoms, there is a cessation of vomiting and purging, which is probably a result of the almost entire arrest of the circulation through the lungs, I believe that venesection affords the only hope of saving life. It is difficult to obtain a stream of blood in these cases; not, as many suppose, because the blood is too thick to flow, but because, in consequence of the block in the lungs, the blood in the veins is nearly stagnant. The bleeding appears to be beneficial, partly by relaxing spasm, and partly by lessening the distension of the right cavities of the heart, and so increasing their contractile power. Repeated doses of ammonia may help to quicken the circulation.

"*Consecutive Fever*.—Reaction from collapse is sometimes followed by a febrile condition—a hot skin, quick pulse, coated tongue, hurried breathing, often a scanty secretion or even a complete suppression of urine, with drowsiness tending to pass into coma. These unfavourable symptoms are more common when, during the earlier stages of the disease, opium and alcoholic stimulants have been freely given; but they may occur when no such means have been employed.

"The best treatment consists in a scanty diet without alcohol, copious diluent drinks, with saline effervescing draughts, an occasional aperient, castor-oil, or sulphate of magnesia or a Seidlitz powder; counter-irritation over the lungs and kidneys, and sometimes local bleeding to relieve congestion of those organs.

"In some cases there is complaint of pain in the region of the stomach during convalescence. This may be relieved by the application of a few leeches over the seat of pain. If, after reaction, the stomach remain irritable, with frequent vomiting, iced water is an agreeable and efficacious remedy."

ART. 24.—*On the relation between Cholera and the Diarrhœa which accompanies it, and the treatment of the latter disease.*

By Sir HENRY COOPER, M.D.

(*British Medical Journal*, June 16, 1866.)

From a review of these important questions Sir Henry Cooper concludes that—

“1. In epidemics of cholera an unusual amount of diarrhœa prevails; it precedes cholera, and extends laterally beyond it.

“2. In the ordinary course of an attack of cholera, diarrhœa is the first symptom, and is undoubtedly the first stage of the disease.

“3. This premonitory diarrhœa is not distinguishable by its history or symptoms from the sporadic or ordinary diarrhœa.

“4. Cases in which diarrhœa has been stayed do not pass into cholera, while those in which it has been neglected may and often do.

“5. The astringent mode of treatment is generally as efficacious in arresting diarrhœa during cholera visitations as at ordinary times.

“6. And as a corollary from the above, it is the duty of those in authority in cholera epidemic seasons to search out and arrest *all* cases of diarrhœa, by the organization of a sanitary police for the detection of the disease, and its treatment in its earliest stages.”

ART. 25.—*Case of Spasmodic Choleraic Disease successfully treated by Hot-water Packing.*

By F. A. BULLEY, F.R.C.S., Senior Surgeon to the Royal Berkshire Hospital, Reading.

(*Medical Times and Gazette*, Aug. 11, 1866.)

Thos. P., aged forty, a county police constable, residing at Maiden Erleigh, near Reading, a temperate man, and previously in good health, was attacked while walking on his beat, about one o'clock P.M., on Thursday, Aug. 2nd, with sudden violent pain in the abdomen, followed almost immediately by tonic painful spasm of the abdominal muscles, and a sensation as if his intestines were being drawn up in knots. It was not accompanied by any particular feeling of sickness, nor had he had any previous diarrhœa. The symptoms had been gradually increasing in severity until Mr. Bulley saw him—about half-past three the same day—when he found him writhing with the most excruciating pain. The abdominal muscles were universally hardened and contracted by spasm, somewhat resembling tetanic spasm, but more continuous and unrelaxing, and extremely sensitive to the touch. The most intense pain was just in the epigastric region, extending backwards to the spine, as if the diaphragm was affected with the spasm. He was in a cold, clammy perspiration, pulse feeble, and he was beginning to feel ex-

tremely faint. Mr. Bulley immediately ordered the abdomen to be fomented, and the flannels, wrung out of water, as hot as he could bear, having been applied three or four times, his body was enveloped in blankets to the number of five or six, the outer one being closely wrapped round his neck, to confine the heat generated by the wrapping, and to take the following draught every two or three hours:—℞ Elixir opii (Newbery's) ℥x.; tinct. capsici ℥v.; olei menthæ virid., olei anisi, āā ℥ij.; confect. aromat. ℥j.; aquæ menthæ pip. ad ℥jss. M. ft. haust. To take a little brandy-and-water occasionally.

On visiting him about two hours afterwards, Mr. Bulley found him lying in a profuse perspiration, which, with the medicine, had much relieved, but not quite removed, the pain. The rigidity of the abdominal muscles had greatly subsided, and he altogether felt much more comfortable.

About seven P.M. he had another attack of pain, but not by any means so severe as the first, for which his wife again fomented him and repeated the medicine, which again relieved him, some little amount of abdominal hardness, however, continuing. About twelve at midnight he had another attack, when his wife again packed him up in the wrappings and repeated the mixture. Shortly after this, while in the perspiration induced, he fell asleep, and on awaking about an hour afterwards he found the pain had left him, the abdominal spasmodic hardness had become quite relaxed, and he felt in every respect much better.

Friday, next morning.—He is quite free from pain and spasm; feels very weak, but the pulse has recovered its strength, with a natural uniform warmth of the body. In the afternoon he was apparently free from all symptoms of the disease, except that he felt somewhat languid and faint. Ordered to take cold strong beef-tea and small quantities of brandy-and-water for drink, and the following mixture should the pain or spasm return:—℞ Chlorodyn. ℥xl.; aquæ menthæ virid. ad ℥vj. M. ft. mist. Take a fourth part if necessary.

Next day the patient had apparently quite recovered from his complaint.

The foregoing case was, Mr. Bulley believes, of the nature of the prevailing epidemic, arrested in its progress just at the point when the suspension of the organic functions was commencing, and which would in all probability have gone on to extreme collapse, if no means had been employed to prevent it; and he felt satisfied that nothing but the previous good health of the patient and his temperate habits enabled him to bear up against the continued and violent spasm so long as he did, without the induction of the more advanced and dangerous symptoms of the disease.

ART. 26.—*On the Use of the Calabar Bean in the Treatment of Cholera.*

By Dr. MAPOTHER, Surgeon to St. Vincent's Hospital, Dublin.

(*The Medical Press and Circular*, September 12, 1866.)

Dr. Mapother reports five cases of cholera, in the treatment of which he used the Calabar bean. He considers that the state of collapse indicates a highly excited condition of the vaso-motor nervous system, inducing spasm of the muscular tissue of the arteries, and consequent constriction of their calibre. The Calabar bean possesses the power of breaking or temporarily paralyzing the vaso-motor influence, and Dr. Mapother thought it was worth a trial in cholera. The results of his observations to the present time are given in the following cases:—

“1. Margaret Devine, aged twenty-eight, was seized with purging, vomiting, and cramps, about three o'clock P.M. on the 5th; she was treated with stimulants until ten o'clock on the 6th, when she was admitted into the Meath Hospital; purging and vomiting had ceased, but the cramps in the legs still distressed her. Neither the radial nor brachial arteries gave pulse, but in the common carotid it counted 92, and felt extremely feeble; the tongue, face, and extremities were very cold, and the surface was almost of a purple plum-colour; voice was nearly inaudible; the pupils were fully dilated. Every one anticipated death within a very few hours. The powdered bean was the only preparation then procurable, and of it three grains mixed in two drachms of water were administered every second hour. The only other treatment consisted in the application of external warmth; vomiting did not recur, nor did purging, but during several hours of the night blood was freely discharged from the bowels. The pupils contracted much after the third dose, and about the same time the coldness and blueness were much decreased. The carotid pulse did not alter in frequency or volume to any considerable extent throughout. At nine A.M. on the following morning she described a sensation of a lump in the stomach in terms very similar to those employed by Dr. Frazer, in describing his own symptoms after a full dose of Calabar bean; at ten o'clock the force and frequency of the circulation and respiration declined, and she sank shortly before midday.

“The intestinal bleeding probably produced, or at least hastened, death; and it may be questioned whether it was due to the very turgid state of the rectal veins, which is usual, or to the action of the drug, which is cathartic.

“2. Michael Shelly, aged fifty, was seized with purging and vomiting early on the morning of the 6th, in a room from which two of his children had been removed to hospital with severe cholera. I saw him there at eleven o'clock, when he had just passed a rice-water stool, and was attacked with cramps in the forearms. There was no coldness or lividity of surface, but the eyeballs were much sunken, and his countenance was terror-stricken. The pupils were small, a condition which

his trade, that of a tailor, may have made habitual. The administration of three-grain doses every second hour was commenced at one o'clock when he arrived at the hospital; but at seven P.M. a tincture made with five ounces of spirit to four ounces of the bean was procured and given in four-drop doses instead of the powder. The purging and vomiting ceased; he remained warm, suffered no cramps, and the pupils considerably contracted. He complained of a peculiar feeling of weight in the stomach, but nothing else, save very great weakness. He remained in this condition during Friday, but on Saturday morning, at nine o'clock, it was found that the rice-water purging and vomiting had returned, and his skin was sensibly colder. He was depressed extremely by the death of his child, which had just occurred in the next ward. As he had therefore got worse under full doses of the drug, I did not feel justified in continuing it, and small doses of calomel were prescribed, and as no urine was secreted, the loins were cupped twice, two ounces of blood being drawn the second time, and a bran-and-turpentine stupe was applied. At nine on Sunday morning he passed water freely, and may be now said to be rapidly recovering.

"3. Patrick Gahan, aged fifty-five, was seen by me at his residence at half-past nine o'clock on Friday morning, 7th. He had had rice-water purging and vomiting; was violently cramped in the legs; his face was pale, the eyeballs much sunken, pupils dilated, his hands and feet cold and bluish, and the radial artery was very thready. On his removal to hospital the tincture was given as in the preceding case, and external warmth was assiduously applied. Two stools were passed during the day, but the other symptoms gradually disappeared, and on Saturday he was almost fit to be discharged.

"4. William Crutchfield, aged fifteen, at about four o'clock on Friday morning was severely purged and vomited, and fainted after one severe fit of purging. He was admitted at half-past twelve with these symptoms and with cramps, but no positive coldness or weakness of pulse.

"The tincture was administered in the same way; the purging and vomiting gradually ceased, and he became warmer; the pupils fully contracted. On Saturday morning he was removed by his father, some prostration alone remaining. He has since remained quite well.

"A fifth case was admitted in full collapse three hours since, and after the second dose considerable improvement was manifested, but the details must be reserved for a future report.

"The above cases," adds Dr. Mapother, "are too few for any positive conclusions to be drawn, but they are published in order that any physician who thinks that my reasons for suggesting the drug are sound may give it a trial on a larger scale. It may be better to administer the medicine by subcutaneous injection, as absorption is weakened, in larger doses than I have done, or combined with stimulants, anti-spasmodics, disinfectants, astringents, eliminants, or any other class of drugs which have been apparently useful."

ART. 27.—*On the Use of Iced Water in the Treatment of Cholera.*

By Dr. DUNCAN MENZIES, Deputy Inspector-General of Hospitals.

(*The Lancet*, June 23, 1866.)

From an extensive experience in India, Dr. Menzies speaks highly of the utility of iced water, *in small portions*, in the treatment of cholera. "I am aware," he says, "that iced water has been frequently employed in the treatment of cholera, but I think without sufficient consideration as to its action and effects. When given, as is frequently the practice, *ad libitum*, it is very apt to disagree, and be directly afterwards rejected, from its accumulated bulk and weight occasioning a sense of oppression and sickness at the precordia; whereas, when the water is taken in the sparing proportions I have recommended, this inconvenience is avoided, and the sufferers will, on the contrary, tell you that they feel much relieved and refreshed after each draught; and this goes on until the stomach regains its normal tone, when medicines will not only be better borne, but also absorbed. I may add that I put this treatment into practice during my stay in the upper provinces of Bengal, which extended over a period of six years, and found it very successful, whether the case was seen early or late in the disease. In some cases there was no previous ailment; in others the attacks had been preceded by diarrhoea."

ART. 28.—*On the Treatment of 123 Cases of Cholera in the Liverpool Parish Infirmary, July and August, 1866.*

By J. WILSON M'CLOY, M.D., &c., Resident Medical Officer at the Liverpool Parish Infirmary.

(*The Lancet*, August 18, 1866.)

The first cases (two) were brought to this hospital on the 10th of July. Both were in the evacuation stage, and were treated with astringents, stimulants, and ice-water. The astringent used was a mixture containing spirits of chloroform, Battley's sedative solution, creasote, and compound chalk mixture. The stimulant was brandy, freely and frequently administered. Ice-water was given *ad libitum*. The symptoms of collapse rapidly set in, and both cases proved fatal: one in twelve, and the other in six, hours after admission.

On the 12th of July the disease made its appearance in the foundling department of the institution. This was one of those sporadic, or at least unaccountable, cases which we occasionally meet with. A nurse in one of the foundling wards, who had not for months been out, or in communication with any one from without, was suddenly and unnac-

countably seized with violent vomiting, painless, profuse purging, and violent cramps in the extremities. The case was considered one of cholera. The woman was removed at once, the place thoroughly disinfected, the bedding, &c., burnt, and the children transferred to a separate ward. This woman was treated in a similar way to the former cases, and with the same result, death occurring in twelve hours after admission.

The same night two of the children to whom this woman was nurse, and who slept with her, were seized with choleraic symptoms. They were treated with camphor, according to the "Rubini" plan. Both cases proved fatal, one in six, and the other in eleven, hours.

The following morning four other children, also charges of this woman, were seized. The camphor treatment was adopted, and three cases proved fatal.

From this time till the 26th of July there were 56 entries. Of these 5 were moribund on admission—dying in from two to seven hours. We have then a total of 51 cases treated up to the 26th ult. Of these 19 were by camphor, 7 by ice, and 25 by what M'Cloy calls the "mixed plan." The following are the results:—

Cases.	Mode of treatment adopted.	Deaths.
5 . .	(Moribund on admission)	5
19 . .	Camphor ("Rubini" plan)	13
7 . .	Ice to spine and ice-water	7
25 . .	Mixed treatment	13
—		—
56		38

Only seven of these fifty-one cases were in the stage of collapse, the rest were in the evacuation stage. In estimating the value of the camphor treatment, it is only fair to state that it was principally pursued amongst a most unfavourable class of patients. Dr. M'Cloy alludes to those puny, rough-skinned, pot-bellied, emaciated children, so common in the lower ranks of life, and in the foundling department of workhouse infirmaries. Ice to the spine, either alone or alternated with hot-water bags, was miserably unsuccessful. The application did not seem to have the slightest effect in producing reaction where there was any considerable collapse. While the ice-bags to the spine were borne without complaining, a similar application of water at 120° Fahr. caused the greatest pain. The mixed treatment included the use of astringents, sedatives, stimulants, ice, ice-water, the hypodermic use of morphia, hydrocyanic acid, strychnine, and camphor, dry heat, sinapisms, stupes, &c. The astringent mixture, which was the same as that used in the first cases, speedily arrested the vomiting and purging; but this was not followed by any general improvement. Dry heat and sinapisms proved beneficial. Brandy and ice-water were administered freely.

On the evening of the 26th the castor-oil treatment was first ventured on as a sort of forlorn hope.

Since the 26th July there were 67 cases under treatment. Of these, 11 were moribund, dying in from ten minutes to eight hours subsequent to admission. This leaves 56 cases, which were thus treated:—

Cases.	Mode of treatment adopted.	Deaths.
11 . .	(Moribund on admission)	11
2 . .	Internal administration of strychnine.	2
4 . .	Astringent and stimulant	4
50 . .	Eliminative	17
—		—
67		34

The two cases in which strychnine was administered were just in the transition stage between evacuation and collapse. The dose was one-thirtieth of a grain every fifteen minutes, with permanganate of potash and carbonate of soda. The astringent and stimulant treatment was that previously noticed. In the remaining fifty cases castor-oil was used. "With the results," writes Dr. M'Cloy, "I have every reason to be perfectly satisfied. Of these fifty cases, only ten were in the stage of evacuation; and of the remaining forty, nineteen were in a state of the most extreme collapse. I observe in the *Pall Mall Gazette* of August 4th, a statement to the following effect: 'The cholera at Liverpool is evidently subsiding, and, as usually happens in such a time, the larger proportion of recoveries is attributed to the mode of treatment, castor-oil having been substituted for camphor and ice.' Now, exactly the opposite of this is the case. *The disease is not subsiding; choleraic diarrhœa is increasing rapidly, and the cholera type is more severe.** It cannot be said that the cases treated on the eliminative plan were milder in character than those treated by camphor, astringents, or ice, for, so far from this being the case, I can most unhesitatingly affirm that they were not only *more severe in character*, but were not, as a rule, prescribed for until collapse had for some time set in. Of the seventeen deaths, two occurred from pneumonia during convalescence; two were cases which had been discharged cured, and were suddenly seized with a relapse; and nine were cases in which there was no radial pulsation, *and in which neither emesis nor purgation could be produced.* . . .

"The method of administration of the castor-oil was, in the majority of cases, that advised by Dr. Johnson in his work on Epidemic Diarrhœa and Cholera. I have found in nearly every instance a wonderful tolerance of this medicine. The most difficult point in the whole treatment of the disease I believe to be that connected with diet, more especially during the stage of convalescence. From want of proper attention to this point, I believe four cases relapsed, two of which died, and two recovered under the castor-oil treatment. From having watched the effects of alcoholic stimulants in collapse, I am of opinion that they invariably diminish the force and frequency of the pulse, and augment the symptoms arising from pulmonary obstruction. Thermometry, so far as I could judge, afforded no measure of the intensity of the collapse. In every case the temperature of the body rose one or two degrees after death. The 'rice-water' evacuation has not been at all a characteristic symptom. The discharges presented every variety in appearance. The peculiar character of the voice, the *facies cholericita*, and the incessant thirst,

* A reference to the reports of the Medical Health Officer will settle this point.

have been the best marked and most characteristic signs. While in many cases the attack came on suddenly and unaccountably, in the majority there were 'premonitory diarrhœa' and abdominal uneasiness. The cases have been of every degree of severity. The disease, as a rule, has only occurred in the low-lying districts, where the unhygienic conditions connected with food, filth, misery, overcrowding, and intemperance, exist notoriously. *The eliminative treatment has been most successful.* It has been a success which those only who have seen and compared the relative severity of the cases can appreciate—a success which statistics cannot show."

ART. 29.—*On the Treatment of Cholera.*

By Dr. A. CLARKE, Physician to the London Hospital.

(*London Hospital Reports*, vol. iii., 1866.)

The following is a summary of the results of treatment of cases of cholera, under the care of Dr. Andrew Clarke, during the recent outbreak. Subjoined are the formulæ referred to in the summary:—

Mistura Astringens.

Decoct. hæmatoxyli	Half an ounce.
Æther. sulph.	10 minims.
Acid. sulph. arom.	15 „
Camphoræ	2 grains
Pulv. capsici	Half a grain.

Every fourth hour.

Mist. Antim. Tart.

Pulv. antimon. tart.	2 grains.
Magn. sulph.	Half an ounce.
Aquæ	Half a pint.

Half an ounce every half hour.

Lead Pill.

Plumbi acet.	2 grains.
Camphoræ	A grain and a half.
Ext. opii.	One sixth grain.
Creasoti	One sixth minim.

One pill every hour.

Mistura Quinæ c. Ferri.

Quinæ sulph.	1 grain.
Tinct. ferri mur.	15 minims.
Aquæ	One ounce.

Every two hours.

The following table refers solely to treatment in the *stage of collapse*.

When reaction began the treatment was modified according to individual peculiarities:—

Treatment.	No. of Cases.	Died.	Living.
Mistura astringens	48	31	17
Mistura rubra (water and sugar)	56	28	28
Castor-oil	21	14	7
Saline lemonade	20	6	14
Mist. antimon. tart.	2	2	0
Mist. quinae c. ferri	3	1	2
Lead pill	9	4	5

“The unenviable position which the *Mistura astringens* occupies in the list,” observe the reporters of Dr. Clarke’s treatment, Mr. J. McCarthy and Mr. Dove, “may be in part, if not altogether, due to the fact that all the cases at the commencement of the epidemic, when the type of the disease was unquestionably worse, were placed on that treatment. This explanation seems the more probable, as at the Wapping temporary cholera hospital this mixture was more successful.

“In very bad cases the *Mistura astringens* and castor-oil, after a few doses, produced such loathing, that the use of them had to be discontinued.

“In the list of recoveries are included several mild cases of true cholera; and two patients, who were removed by their friends, of whom one was ascertained to have afterwards died; but at least between fifty and sixty were extremely bad cases, in which either in collapse, or in reaction, the prognosis had been unfavourable

“The *Mistura quinae c. ferri* was not used until a late period of the epidemic, but many of the cases still in the hospital were treated with it, and are doing remarkably well. Although it shares the fate of all other medicines, in being *apparently* all vomited up as soon as swallowed, yet some is retained and absorbed, as is proved by iron being found in the urine, and by the colour of the discharge from the bowels.”

ART. 30.—*On the Use of Warm Baths in Cholera.*

By Dr. A. CLARKE, Physician to the London Hospital.

(*London Hospital Reports*, vol. iii., 1866.)

The use of warm baths in the treatment of cholera cases admitted into the London Hospital during the recent epidemic, would appear to have been followed by more beneficial effects than are commonly attributed to this method of relief in the disease. “Baths at a temperature of from 98° to 104° Fahr.,” say Dr. Clarke’s clinical reporters, “were given, with most marked effect, in about one hundred and thirty of the worst cases. In almost all the cases there was commonly, for a few seconds, difficulty of respiration; and in many, for about the same period, an unpleasant sensation of heat. In children, fright also contributed in causing some difficulty; but generally, in less than a minute,

the good effects of the bath became manifest. Cramps ceased, anxiety of mind vanished, pulse returned, or if originally to be felt, increased in volume and frequency. Many who had before moaned or shouted incessantly with pain, began to converse upon indifferent subjects, or in many cases sank into a tranquil slumber. Often recovery appeared to be the direct consequence of the bath, the improvement being permanent; but in many more, removal from the bath became the signal for the return, more or less rapidly, of the former symptoms.

"The testimony of all who had a fair opportunity of judging, is unanimous as to the relief afforded by the warm baths, the most convincing being that of the patients, who, in some cases, craved incessantly for them, and remained in, at their own request, for nearly an hour at a time.

"In a very few cases no relief was derived, but those were cases of great collapse, where their employment had been dictated by despair, rather than by any hope of benefit."

ART. 31.—*On the Treatment of Cholera by the Injection of Fluids into the Veins.*

By Mr. LITTLE, Assistant Surgeon, London Hospital.

(*London Hospital Reports*, vol. iii., 1866.)

Fifteen cases of cholera admitted into the London Hospital during the recent epidemic were treated by the introduction of fluid into the veins. Only patients "with no apparent chance of recovery" were injected; "cases of extreme collapse, all of them pulseless at the wrist; livid, with low external temperature, and having lost quantities of fluid, either by purging or vomiting, generally by both." In two of the cases, defibrinated sheep's blood was injected; in other two, serum of sheep's blood; and in the remainder, a saline fluid, which was constituted as follows:—

Chloride of sodium	60 grains.
Chloride of potassium	6 "
Phosphate of soda	3 "
Carbonate of soda	20 "
Distilled water	20 ounces.

In most of the cases, and in all the successful ones, two drachms of pure alcohol to the pint of water were added, the proportion used successfully by Dr. Little in 1849. The fluid was injected at a temperature of about 110°; in the earlier cases by a special syringe, subsequently by gravitation.

Of the *fifteen* cases, *four* recovered, in all of which the saline fluid, to which alcohol was added in the proportion stated, had been used.

In addition to Mr. Little's account of the treatment of cases by injection of fluids into the veins during the recent epidemic, the same

volume of the London Hospital Reports contains an important paper on the treatment of cholera by this method, contributed by Dr. Little.

ART. 32.—*On the Physiological Formula of Cholera and of the Treatment.*

By B. W. RICHARDSON, M.A., M.D., F.R.C.P., Senior Physician to the Royal Infirmary for Diseases of the Chest.

(*Medical Times and Gazette*, August 4, 1866.)

Dr. Richardson sustains the following theorems respecting cholera, and founds upon them certain definite rules of treatment:—

1. The symptoms of cholera are due to the separation of water from the albumen of the blood and of the tissues.

2. The separation of water from the blood in cholera is due either to a local osmotic change in the alimentary canal, or to a general osmotic change in the blood itself.

3. The collapse of cholera is due not only to the elimination of water from the system, but to the removal by the water of the heat of fluidity or latent heat of the tissues.

From these theorems it follows that there are three lines of practice open in cholera. These are, with their application, as follows:—

1. To arrest elimination.
2. To supply the caloric of fluidity.
3. To restore the homogeneousness of the blood.

If we could see a certain and simple way of restoring the homogeneousness of the blood, we should have in our hands an immediate antidote to cholera, and the third suggestion would become the first and only suggestion. Unfortunately we have yet, by continued experiment, to learn this antidote, and we must, as a consequence, reserve the trial of it for the extreme stages of the disease—a last resource.

To fulfil the first of these indications—viz., to arrest exudative discharge—there are only two sets of remedies known: opium, which acts, when it can be absorbed, on the involuntary nervous system, producing contraction of the capillary vessels of the alimentary surface (in the same manner as it produces contraction of the pupil); and direct local astringents, which act on the secretions of the canal—viz., creasote, tannin, the mineral acids, and some metallic salts. It must be clear that none of these are curative remedies in the extreme sense of the term—that is to say, they are not direct remedies or antidotes for the primary derangement. But by their astringent action they prevent the water of the body from being conveyed away, and in this sense they conserve the animal caloric represented in the water, and which is absolutely lost when the water actually flows from the organism.

To fulfil the second indication, to sustain the caloric, there are two methods open. One of these is to prevent, as far as is possible, the radiation of sensible heat from the body. The cholera patient should

be treated in the same manner as a man who is exposed to extreme coldness of the air. His animal fire low, and the conveyance of caloric in his system interfered with, the choleraic sufferer is the parallel of an Esquimaux exposed without shelter to polar air and deficiency of food. To expose a man reduced in temperature to any process that shall remove from him caloric, is contrary to all reason; to give him the advantage of the hot bath and to maintain his temperature by efficient clothing is the most natural, as it is the most obviously useful procedure. One has only to see a few times how the physical forces of life improve when the patient, collapsed and cold, temporarily rallies in the hot bath, to be assured of the soundness of the practice. This, however, is not again a curative process in the direct sense, but conservative only; for the body, by its external surface, cannot absorb caloric unless there be actual destruction of its surface.

It is still a desideratum to supply animal caloric, and this, Dr. Richardson points out, may be accomplished to a marked degree by attention to the fluids which are given as drinks to choleraic patients. He denounces as utter folly, and almost worse, the practice of charging such patients with cold and iced drinks. Presuming that a choleraic sufferer is just sustaining his natural caloric at 96° Fahr., he does this by the gradual consolidation of his tissues and the giving up from them of their heat of fluidity. At this stage let there be given to him a pint of fluid at 40° Fahr., and straightway, from an equal weight of his body, there is extracted by equalization 28° Fahr. of caloric, which, as he is placed, will never again be applied for the production of force.

This plan of cooling down a cooling body is, according to Dr. Richardson's estimate, adding evil upon evil. To treat the disease rationally, the reverse principle ought to be invariably carried out; that is to say, foods and drinks should be made the means for introducing heat abundantly. It seems to him, on this reasoning, to be an important point to produce a substance which shall, as a liquid food, supply tissue material and with that heat.

Taking advantage of the fact that crystallizable fat, when mixed with albumen, can be dissolved by the heat of water, which heat it fixes in becoming soluble, and gives up again on solidifying—Dr. Richardson set to work to produce a food having the properties named. After numerous attempts the following proved most successful:—

Take of pure stearine two ounces by weight; of best fresh butter, two ounces; of whites and yolks of eggs, well beaten up, eight ounces; of carbonate of soda, twenty grains; of chloride of sodium (common salt), eighty grains; of water, two ounces.

As to mode of preparation for food. First dissolve, with heat, the stearine and the butter until they are both melted; then add the carbonate of soda and common salt to the eggs, and when these salts are dissolved in the egg stuff, mix it with the oily fluid, taking care that the latter is not of a temperature above 140° Fahr.; let the whole cool to a soft consistency, and finally, on a slab or a board, rub in the water with a broad spatula. The compound may now be placed in a wide-mouthed jar; in a little time it settles into a moderately-hard mass, and is ready for use.

In administering this compound to the sick, take one ounce, or a table-spoonful; place it in a large breakfast cup, and rub it up equally

with a teaspoonful of glycerine, or a teaspoonful of ordinary water, or a teaspoonful of fine sugar and water, or a teaspoonful of honey and water. Next pour upon the mass three ounces of water, *actually boiling*, and incorporate well. The solid substance will now briskly and evenly dissolve, and will be at once so cool that it can be drunken. The thermometer plunged in it will only register from 130° to 135° Fahr. In this process the heat of the boiling water has been mainly (allowance must be made for conduction and radiation) expended in rendering fluid the solid matter. We may estimate safely, that in addition to the sensible heat, 44° have been rendered latent for every ounce of fluid at least, which heat will be yielded up to the tissues if the fluid make its way into them.

Contrasted with the supply of a pint (pound) of ordinary water at 40° , a pint of this fluid would effect a difference equal to 204° of added heat to a pound weight of the substance of the organism.

Dr. Richardson adds, that the fluid food prepared as proposed above is very agreeable to the taste, and that it may be made the vehicle for conveying either solution of opium, creasote, or dilute sulphuric acid, in proper doses. He found after taking nine ounces of the fluid no sensation of nausea or uneasiness, but in the course of half an hour, the surface of his body became very hot, and the heat increasing, remained unpleasantly high for several hours. For patients who would object to drink the fluid while heated, it might be allowed to cool ten or even twenty degrees. A glass of port wine is easily miscible with four ounces of the fluid.

The last indication of treatment is to endeavour to restore the homogeneousness of the blood, and to bring the red corpuscles into proper circulatory action. This can only be effected by injection direct into the veins, and we know from experience that in the last stages of collapse the injection of certain fluids into the veins has been attended by remarkable results; the collapse has, for the time, ceased, and, in some instances, the appearances of recovery have been so decided that the most sanguine but delusive hopes have been raised.

The great question to be settled is—What fluid shall be injected? Blood has been injected and has failed; milk has been injected, and has usually failed; saline solutions have been injected, and have, as a general rule, failed; simple warm water has been injected, and has, in its turn, failed. In short, none of these solutions have been potent in saving life, but one and all of them have, for a time, averted death.

The reason why certain immediate but not lasting benefits have followed these various injections is, that they have always been injected after being heated up to blood heat; the caloric thus supplied has been the underlying basis of the transient success. Hence we must consider the question of transfusion in a new light, or success from it will continue to be only temporary.

In respect to transfusion in the collapse of cholera, there is as little difficulty in accounting for the ultimate failure of the proceeding, as for the temporary success. The success depends on the infusion of caloric, the failure on the inability of the fluids injected to sustain the calorific activity.

What then do we want in the way of a fluid? Dr. Richardson thinks

that the fluid should contain digested and easily hydrating albumen; fatty substance that would solidify at a low temperature; a small quantity of saline matter, and a substance that will easily pick up the blood corpuscles, when they have been partially deprived of fluid, and restore them to their natural form and character. He has worked upon drying blood to test for such a fluid, and at last has arrived at the following formula, which, on blood out of the body at all events, answers the purpose well. He gives the proportions for a pint of fluid.

Of white of egg take 4 ounces by weight; of common salt, 1 drachm; of carbonate of soda, 1 scruple; of clarified animal fat, 1 ounce; of pure glycerine, 2 ounces; of water, sufficient to make 1 pint. In preparing, dissolve the common salt and carbonate of soda in the water, and having well whipped the albumen, add that also to the water. Place the mixture on a water bath, and raise the temperature to 135° ; keep the mixture steadily stirred and digest at this temperature for one hour. This constitutes an artificial serum, the albumen of which hydrates freely. Having taken the artificial serum off the bath, place the fat and the glycerine together in a crucible, and melt the fat in the glycerine. When the process of solution of fat is complete, pour the solution into the artificial serum at or about 120° , and stir in carefully; set aside that the fluid may cool to 80° Fahr., at which point all the fat that is insoluble at 80° will float on the surface, take this off and filter through coarse paper or closely-woven cloth.

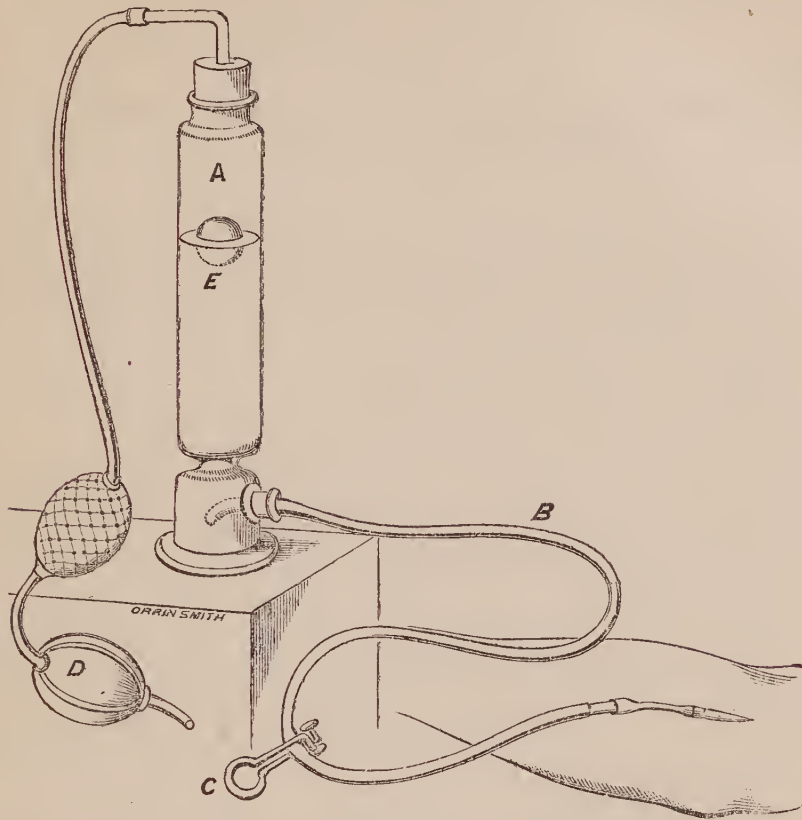
The fluid thus obtained is of pinkish colour, of alkaline reaction, of saline sweetish taste, and of specific gravity, 1038. It picks up semi-fluid blood with instant readiness, and diffuses it most equally. Heated, it takes up one-third more caloric than does water in the same time, and in cooling it restores nearly one-third more.

From his experience with mere saline injections, Dr. Richardson infers that the fluid might be injected, at a temperature of 106° , into the veins of a collapsed adult, in the proportion of two pints at one time.

In reference to the mode of injecting the veins, Dr. Richardson observes that the greatest mischief has arisen in transfusion, from errors in the operation. The operator should ever remember that in this process he is feeding, not forcing: he should keep in mind how gradually nature feeds the veins by the thoracic duct, and he should imitate her; there is no necessity for force, none for hurry.

Above all things, in feeding by the veins the syringe should be thrown aside: it is a dangerous, bad instrument for the purpose. To replace it he has constructed a simple instrument, which he has used with great success. This instrument, described best in the subjoined sketch consists of a glass cylinder (A), with a flexible tube (B) running from its lower part or chamber for insertion, by means of a quill, or hollow probe, into the vein to be injected: the upper part of the cylinder is provided with a stopper, through which a tube passes, connected with a small pair of hand-bellows (D). Within the cylinder is a small hollow ball (E), or safety valve regulator, which floats if there be fluid in the cylinder until the fluid allows it to descend to the constricted lower part of the cylinder, when all further passage of fluid is prevented. The flow of fluid along the escape tube can be checked, or set at liberty at pleasure by the clip (C).

In using this instrument, first place the warmed fluid to be injected in the cylinder (A) and let a little run through the escape tube (B) to



displace all the air; next close the escape tube by means of the clip (C); then, having opened the vein while it is being pressed upon from above, insert and fix the quill or hollow probe at the end of the escape tube, and when all is ready for the fluid to flow, remove the clip and raise the cylinder two or three feet above the patient. The ordinary fluid pressure will now usually suffice to carry the fluid into the body equably and gently; but if there be any obstruction, the merest pressure of the lower ball of the hand-bellows will remove it. As the fluid descends, the hollow ball goes down with it to within three inches of the bottom of the cylinder, where it is opposed by the constricted neck, and where it effectually closes in all that is below it, so that no air can possibly get into the vein.

This instrument works so easily and effectively, that with it Mr. Gay and Dr. Richardson injected the whole body of a dead subject with blood by the femoral artery until the fluid escaped from the nostrils; it acts equally well on large living animals.*

* Messrs. Krohne and Sesemann manufacture the instrument, with all the details shown in the diagram.

ART. 33.—*On the Physiological Conditions which avert a fatal result in Cholera.*

By JOHN GEORGE FRENCH, F.R.C.S., Surgeon to the Infirmary of St. James's, Westminster.

(*Medical Times and Gazette*, July 21, 1866.)

Mr. French holds, from a study of this subject taken from the point of view which is afforded by the recovery of patients from the severest forms of the disease, that the circumstances which avert the fatal effect of this poison are the following :—

1st. Diminution of the heart's action.

2nd. Diminution of the aëration of the blood.

3rd. Excretion of the poison by the alimentary canal.

The diminution of the heart's action is, in other words, the arrest of the circulation of the poison. In practice we adopt this principle, in the examples of venomous bites, by the ligature and other contrivances to prevent the entrance of the poison into the circulation.

Next of the diminution of the aëration of the blood. This has been mistaken for the condition known as asphyxia. Dr. Edmund Parkes has, however, very clearly pointed out a material difference between the two conditions. Thus, he says, "In asphyxia the cause is the absence of a respirable gas. In cholera there is some condition which prevents the blood from submitting itself to the action of the air." Mr. French believes that herein lies the true distinction. Thus, instead of being misled by the analogies of syncope and asphyxia, from which the points of difference are far stronger than those of resemblance, the more reasonable assumption is that poison, the nature of which is to destroy life, is so bad a thing to circulate in the blood that, while it exists in the blood, the less circulation there is of it the better; and this explains the remedial influence of—1st. The diminution of the heart's action; 2nd. The diminution of the aëration of the blood. We now have to consider that part of the remedial process which is assigned to the alimentary canal.

Were it only necessary to relieve the congestion consequent on the abnormal condition of the thoracic viscera, we might regard the profuse evacuations of the alimentary canal as an obvious method of relief; indeed, in certain structural obstructions of the heart's action, we use the elaterium to afford relief by producing similar evacuations, but in cholera we have by this time a direct elimination of the poison itself, which, when eliminated, results in a gradual restoration to health.

The most direct proof that the elimination of the poison is really effected by this process is the fact that the drinking of water contaminated with the excretions is capable of giving the disease to any number of people who swallow it.

If, indeed, the poison of cholera is of such mortal tendency that the thoracic viscera cannot safely perform their natural functions, we need not be surprised if a perfectly abnormal duty should be assigned to the

alimentary canal, and such, in fact, is the case. Instead of performing its natural function of elaborating fresh material for the supply of the waste of the system, it is wholly and energetically engaged in throwing off the poison from the system.

When the poison is expelled, the mechanism of reaction from the state of collapse is carried on by the absorption of water into the blood-vessels, and by the action of vomiting which supplies the place of the *vis a tergo*, but which in the normal circulation of the blood is supplied in another way.

With such a distinct and consistent theory as this to guide us, Mr. French believes as satisfactory a course of treatment may be pursued in cholera as any whatever known in the practice of medicine, not excepting even such remarkable exhibitions of skill as are evinced in the treatment of syncope from all its varied sources, or the best understood mechanical injuries where no mistakes whatever are made — never forgetting that the worst cases of all diseases and injuries are inevitably fatal.

In the clinical treatment of cholera that which most contributes to the recovery of the patient is attention to all those circumstances which most refresh him—the free use of iced water, the most grateful of all, and which, while it assuages a marvellous thirst, requires no digestive power, which is utterly absent. Seltzer water is also free from objection. Strict attention should also be paid to the feelings of the patient with regard to temperature. It is most unwise to submit him in any way to disagreeable heat, which materially adds to his sufferings and danger.

ART. 34.—*On the Feeble Pulse as an Index of Treatment.*

By MR. J. C. SKEY, F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

(*Medical Times and Gazette*, July 21, 1866.)

On this subject Mr. Skey remarks:—"I consider the treatment of the great majority of diseases to consist in increasing the quantity of healthy blood and giving force to the action of the heart. *You can't cure disease with a feeble pulse.* Mend the pulse, and Nature will do the rest of the work. On this principle disease in general may be treated, so far as my observation has gone, with pre-eminent success. In order to appreciate fully its force, you must start with the conviction that Nature cures and not man—man removes obstructions from her path, and nothing more. This done, he awaits the onward move of the great machine, like to a great ship of gigantic weight, which, quietly held in her position at rest by a few timbers, immediately obeys the great natural law of gravitation on their removal, and glides into the water below. Did man launch this vessel, or did Nature? With as much title may the Physician or the Surgeon declare that he cured a disease. There are of course occasional exceptions to this assertion in some cases of operative Surgery. The object of treatment is to restore the pulse to its normal standard of

force and frequency. Give it due force, and the heart will determine the number. As a rule, in cases of debility, it is too frequent, and frequent because the quantity of blood in the system is below the standard of health. Increase the quantity, and the pulse falls. Assure yourselves of this. Unthinking persons jump to the conclusion that brandy or other stimulants necessarily raise the pulse, but this supposes that we start with a healthy pulse at par. I am talking not of health but of disease. In my capacity of Examiner at the College of Surgeons, I often put this question:—If you take a pint of blood from a healthy man of 40, with a standard pulse of 68, what effect will be produced on the number of pulsations by the loss? What do you imagine is the frequent reply? ‘It reduces the number to 60!’ And this curious answer explains something of the phenomena of venesection so universally practised some years ago, when in reporting on a case it is said ‘His pulse rose on bleeding, and so I bled him again.’ As a rule, you will find that whenever the frequency of the pulse is above the standard of health, *as an indication of debility*, a stimulant will reduce it. I tried this experiment, or rather I obtained this test, for it was not an experiment. On coming out of a Turkish bath of something more than the usual intensity of heat, my pulse had risen to 90; I drank about two ounces of wine, and my pulse fell to 75 within a few minutes.”

SECT. II.—SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 35.—*Softening of the Cerebellum.*

By Dr. E. VENTURINI.

(*Revista clin. di Bologna*, 1865; *Schmidt's Jahrbücher*, 1866.)

A shoemaker, twenty-nine years old, of melancholy and sluggish temperament, a large eater and drinker, but without sexual inclinations, had suffered for more than a year, without any known cause, from occipital pains of increasing frequency and severity. At last these pains returned almost daily, and even several times a day, the attacks lasting from half an hour to an hour, and compelling the patient to lie down, with closed eyes and head supported. In May, 1862, after much exposure to the sun, the pain became continuous, and was soon attended by fever, and by unquestionable evidence of meningitis. The pain was most severe in two points, the forehead and the occiput; and extended down the neck, which could only be moved with difficulty. The patient became sleepless, restless, and constipated, with a hot skin but normal pulse (70). On the fifth day appeared strabismus, vomiting, and hiccup, and afterwards sopor and difficult deglutition, the intelligence remaining normal. The pulse sank to 40, the trunk and extremities became icy cold, the skin unsensitive to even the strongest irritants, the head spasmodically bent forwards and to the left side. The thoracic and abdominal organs

seemed unaffected, except that the urine became at last somewhat albuminous. All remedies—bloodletting, ice, calomel, blisters, &c., &c., were unavailing, and the patient died on the sixteenth day of the acute disease. The autopsy shewed the dura mater firmly united to the skull, and the vessels distended by blood; at the base of the skull a little serum. The arachnoid, both on the convexity and at the base, was patched with purulent deposit, which was less abundant on the cerebellum. The pia mater was bright red, easily separable from the brain substance. The brain showed numerous bloody points in section, was altogether somewhat soft, and the ventricles were distended by serum. The cerebellum was less vascular on its surface, and less covered by exudation than the cerebrum. The left lobe contained in its posterior part a fluctuating translucent cyst-like spot, of the size of a cherry; its outer wall formed by transparent arachnoid, its base by softened brain substance. It contained a clear serum, and involved and entirely destroyed the ciliary body. The corresponding part of the other half of the cerebellum was normal.

This case shows the correctness of the experimental conclusion that the co-ordination of movement is disturbed by affections of the cerebellum, since the patient was not able to stand erect. Cases in which the co-ordination remains undisturbed, prove only that the still uninjured part of the cerebellum, or even the cerebrum, may govern the function; and, on the other hand, cases of disturbed co-ordination with no cerebellar lesion may be attributed to the propagation of morbid action from distant parts. The symptoms of meningitis in the case related were probably unconnected with the softening, since there was present a sufficient cause of the meningitis, in the exposure to the sun's rays. Moreover, the anatomical signs of meningitis were less apparent over the cerebellum than in other parts of the brain. No changes were discovered in the arteries, so that the softening could not be considered a necrosis, and must have been the result of a chronic inflammatory process, the cause of which remains unexplained.

ART. 36.—*On the Use of Capsicum in Delirium Tremens.*

By Dr. LYONS, Physician to the Whitworth Hospital, &c.,
Dublin.

(*The Medical Press and Circular*, June 20, 1866.)

The following case is related by Dr. Lyons in illustration of the efficacy of capsicum in the treatment of delirium tremens:—A tavern-waiter, of chronically intemperate habits, was admitted to the Whitworth Hospital in the first stage of this morbid condition. The patient exhibited tremor in almost all the muscles of the body, chilliness, debility, sleeplessness, foul tongue, severe and general uneasiness, but there was a total absence of illusions, horrors, or delirium to any degree. He got a single dose of capsicum, twenty grains in a bolus, after which he slept and fully convalesced, the disease having been thus peremptorily cut

short. Dr Lyons remarks on the great importance of this early phase of the disease being recognised and promptly treated. The patient is in that condition in which he may be by but slight further indiscretion plunged suddenly into all the horrors and moral degradation of the state of fully developed delirium tremens, with all its consequent loss of character with others, and loss to the patient himself of that last barrier against utter abandonment, the sense of shame and remorse.

As Dr. Lyons observes, a brief but variable period often precedes the fully developed attack of delirium tremens, especially in first cases, in which the patient presents anomalous symptoms unintelligible to himself, and not always read aright by his attendant. This stage is in some patients marked by the occurrence of tremor, sleeplessness, and general distress and anxiety, without a trace of delirium. In other instances slight illusions prevail without tremor, from which the patient can by an effort arouse himself, and under strong self-directed exertion of the will even command his faculties for a time, and pursue avocations of business, to break down, it may be, hopelessly, a few hours subsequently, if his condition is neglected, misunderstood, or mistreated. Under these circumstances the treatment by capsicum comes in very opportunely, and by its employment we may, as in the case just cited, cut short the disease, and so save the patient from the consequences of his imprudence, and possibly restore him to a reformed life. Another case well illustrates the success of this drug when opium had completely failed to alleviate the symptoms, and seemed on the contrary in many respects to aggravate the patient's condition. The case was that of an individual who had taken six grains of opium within a period of two or three days without sleep being procured, or any relief to the illusions, tremor, and distress under which the patient laboured. After a twenty-grain dose of capsicum in bolus, profound and refreshing sleep for twelve hours was induced, and the patient awoke conscious and restored. In an almost precisely similar instance occurring about the same period a thirty-grain dose of the drug had to be given a second time before full relief was procured. In one or two instances of individuals of confirmed and extremely intemperate habits it was found necessary to repeat the dose some three or four times.

As to the physiological action of the remedy, Dr. Lyons's explanation is that already given in a former communication—namely, that it produces a powerful stimulant and sedative influence by its direct action on the gastric filaments of the vagi. Slight uneasiness in the stomach has been complained of in one instance only after its use, and in two instances somewhat smart purgation was noticed, but without any evidence of intestinal or other irritation.

As at present employed, the drug is administered in bolus made up with honey of roses; but Dr. Lyons suggests the feasibility of its being conveyed to the stomach in the more agreeable form of a capsule.

ART. 37.—*A Case of Atrophy or Degeneration of the Muscles of the Upper and Lower Extremities from Disease of the Spinal Cord.*

By Mr. GEORGE LEWIS COOPER, F.R.C.S., Surgeon to the
Bloomsbury Dispensary.

(*The Lancet*, July 14, 1866.)

J. J —, aged forty-one years, married, but his wife had no family ; was much exposed to the weather in his daily occupation, at the same time had been a man of intemperate habits, and the subject of a long chronic cough. He was admitted under the care of the author at the Bloomsbury Dispensary on Feb. 14, and died on the 26th. He suffered from complete paralysis of the upper and lower extremities, with atrophy of the muscles of these parts. The symptoms were slow, but progressive. They commenced in the hands and feet, and extended to the arms and legs, and ended in total paralysis. His cough was severe, with purulent expectoration, to the time of his death, which took place on Feb. 26th. The post-mortem showed much detension of the coverings of the cord from fluid, with congestion of the pia mater in the cervical region, and considerable softening in the substance of the white columns. At the commencement of the lower third in the dorsal region the central grey substance contained a large dilated vessel on each side, surrounded by extravasated blood-globules ; and the extremities of the posterior cornua were highly vascular, as also in certain parts of the grey substance there were patches of extravasated blood.

ART. 38.—*Cases Illustrative of Insanity of Feeling and of Action.*

By HENRY MAUDSLEY, M.D. Lond., Physician to the West London Hospital ; formerly Resident Physician of the Manchester Royal Lunatic Hospital.

(*The Lancet*, June 23, 1866.)

A married lady, aged thirty-one, who had only one child, a few months old, was four months afflicted with the strongest and most persistent suicidal impulse, without any delusion or any disorder of the intellect. After some weeks of zealous attention and anxious care from her relatives, who were all most unwilling to send her from among them, it was found absolutely necessary to place her in an asylum, her suicidal attempts were so numerous, so cunningly devised, and so desperate. On admission, she was most wretched because of her frightful impulse, and often wept bitterly, deploring piteously the great grief and trouble she was

to her friends. She was quite rational, even in her horror and reprobation of the morbid propensity; and all the fault which could possibly be found with her intellect was that it was enlisted in the service of the morbid impulse. She had as complete a knowledge of the character of her insane acts as any indifferent bystander could have, but she was completely powerless to resist them. Her attempts at self-destruction were varied and unceasing. At times she would seem quite cheerful, so as to throw her attendants off their guard, and then would make with quick and sudden energy a precontrived attempt. On one occasion she secretly tore her night dress into strips while an attendant was close by, and was detected in the attempt to strangle herself with them. For some time she endeavoured to starve herself by refusing all food, and it was necessary to feed her by means of the stomach-pump. The anxiety which she caused was almost intolerable, but no one could grieve more over her miserable state than she did herself. Sometimes she would become cheerful and seem quite well for a day or two, but would then relapse into as bad a state as ever. After she had been in the asylum for four months she appeared to be undergoing a slow and steady improvement, and it was generally thought, as it was devoutly hoped, that one had seen the last of her suicidal attempts. Watchfulness was somewhat relaxed, when one night she suddenly slipped out of a door which had carelessly been left unlocked, climbed a high garden-wall with surprising agility, and ran off to a reservoir of water, into which she threw herself headlong. She was got out before life was quite extinct, and after this all but successful attempt she never made another, but gradually regained her cheerfulness and her love of life. The family was strongly saturated with insanity. "In face of such an instance," observes Dr. Maudsley, "of uncontrollable impulse—and it is not very singular,—what a cruel mockery to measure the lunatic's responsibility by his knowledge of right and wrong!" The attack in this case was traceable to a strong hereditary predisposition, in conjunction with physical and mental depression arising from the suckling of a child, and from frequent and long absence from home of the husband.

In another case, the morbid impulse, not less desperate, was homicidal. An old lady, aged 72, who had several members of her family insane, was afflicted with recurring paroxysms of convulsive excitement, in which she always made desperate attempts to strangle her daughter, who was very kind and attentive to her, and of whom she was very fond. Usually she sat quiet, depressed and moaning because of her condition, and apparently was so feeble as scarcely to be able to move. Suddenly she would jump up in great excitement, and shrieking out that she must do it, make a rush upon her daughter that she might strangle her. During the paroxysm she was so strong and writhed so actively that one person could not hold her; but after a few minutes she sank down quite exhausted, and, panting, would exclaim, "There, there! I told you; you would not believe how bad I was." No one could detect any distinct delusion in her mind; the paroxysm had all the appearance of a mental convulsion; and had she unhappily succeeded in her frantic attempts, it would certainly have been impossible to say honestly that she did not know that it was wrong to strangle her

daughter. In such event, therefore, she ought legally to have been hanged, though one may doubt whether the juridical farce could have been played out, so palpably insane and irresponsible was she.

"These cases," Dr. Maudsley remarks, "are examples of uncontrollable impulse without manifest intellectual disorder; they properly belong to what might be described as the *impulsive* variety of affective insanity. It is not true, as some have said, that the morbid impulse is the entire disease: the patient's whole manner of feeling, the mode of his affection by events, is more or less perverted, and the springs of his action, therefore, are disordered; the morbid impulse is the outward symptom of a deeper-lying disease of the affective life, which is truly more dangerous than disease of the intellectual life, because its tendency is to express itself, not as intellectual derangement does in *words*, but in *actions*. Man *feels, thinks, and acts*; in other words, has *feeling, cognition, and volition*. The feelings mirror the real nature of the individual, and it is from their depths that the impulses of action come, while the function of the intellect is to guide and control. Consequently, when there is perversion of the affective life, there will be morbid feeling and morbid action, which the intellect cannot check nor control, just as, when there is disease of the spinal cord, there may be convulsive movement, of which there is consciousness, but which the will cannot restrain. The existence of dangerous insanity of action and feeling, without marked intellectual derangement, is in strict accordance not only with the physiology of the nervous centres, but also with the first principles of a sound psychology; it is established also beyond all possibility of question by the observation of actual cases of insanity.

"If we examine into the histories of the patients so afflicted, it will be found in the great majority of cases that there has been a strong hereditary predisposition to insanity, as there notably was in the two cases mentioned. And wherever such innate taint or defect of nervous element exists, we may justly presume that any great commotion in the system, whether produced by external causes, as adverse circumstances or other occasions of mental agitation, or arising from internal causes, as puberty, pregnancy, and the climacteric period, will be fraught with danger to the mental equilibrium."

ART. 39.—*On the Morbid Anatomy of the Nervous Centres in General Paralysis of the Insane.*

By MR. J. LOCKHART CLARKE, F.R.S., &c.

(*The Lancet*, September 1, 1866.)

It is very important, Mr. Lockhart Clarke states, to be aware that in every healthy brain, or at least in every brain that on examination is usually considered healthy, a great number of the capillaries and small arteries are surrounded by secondary sheaths, precisely similar in all *essential* particulars to those which have been considered as morbid products in

general paralysis and other cerebral affections. This anatomical fact was, he believes, first pointed out, about eleven years ago, by M. Robin, of Paris, and was afterwards made the subject of a paper, with engravings, in the second volume of the *Journal de Physiologie*.

Mr. Clarke's observations confirm the general correctness of this description and of the remaining statements of the author. He has found such sheaths around a variable number of blood-vessels in the brains of persons who have died without any apparent cerebral disorder, and one of these brains belonged to a fine, powerful, and healthy-looking young man, who was killed by an accident in the street.

Yet, on comparing vertical sections of the convolutions of a healthy brain with those of a brain from a person who has died of general paralysis, a striking difference between them is often observable even to the naked eye. In the latter case, a series of streaks or lines may frequently be seen radiating through the white and grey substances towards the surface; and in vertical sections of convolutions that have been hardened in chromic acid, it is very common to perceive, in the white substance especially, what seems at first sight to be a number of vertical fissures and oval slits, which, under the microscope, however, are found to contain blood-vessels surrounded by sheaths like those already described. But the sheaths in these cases are often less delicate; they are thicker, more conspicuous, and frequently darker than in the healthy brain; and sometimes, especially when the vessels are convoluted, they appear as fusiform dilatations along their course. Moreover, while in the healthy brain the granules or grains of hæmotosin are commonly scanty, and frequently absent altogether, in general paralysis they mostly abound, being scattered in some places, and collected into groups in others. So much for the state of the cerebral blood-vessels in general paresis. In the nerve-cells of the convolutions Mr. Clarke has frequently discovered certain structural changes, which, as far as he is aware, have not been mentioned by other observers. These changes consist of an increase in the number of the contained pigment-granules, which in some instances completely fill the cell. In other instances the cell loses its sharp contour, and looks like an irregular heap of particles ready to fall asunder.

A French writer, M. Joire, has stated that, during an experience of three years, he has always found in cases of general paralysis a peculiar alteration of structure in the fourth-ventricle of the brain. This alteration consists of the formation of a considerable number of granulations resembling the elevations produced on the skin under the influence of cold. At an early stage of the disease the granulations are numerous and small, and suggest the idea of a surface covered with grains of sand. In older cases the granules are larger, and afford a rough sensation to the touch. They are most remarkable at the point of the calamus scriptorius.

The appearance described by M. Joire is quite familiar to Mr. Clarke, but he has not always found it in general paralysis; and it is certainly not peculiar to this disease, for he found it in cases of an entirely different nature.

ART. 40.—*The Restraining Nerves, a Contribution to Nervous Pathology.*

By Drs. A. EULENBURG and L. LANDOIS.

(*Wien. Med. Wochenschr.*, 1866; *Schmidt's Jahrbücher*, 1866.)

Although until recently only two great groups of nerves were recognised, the sensory and the motor, yet latterly a third group, the restraining nerves, has been added to the former. The elucidation of the diseases of these restraining nerves is the subject of the present paper.

Under restraining nerves are included all those that interrupt, anywhere or anyhow, irritation produced or movement originated elsewhere. Their function, therefore, consists in the staying of movement. These nerves, like all others, are liable to disorder, both in their active and in their conducting apparatus. Besides these central ganglia and fibres, the restraining system possesses also special peripheral terminal organs, to which the restraining impulse excited in the centre or in the conductor is directed, and in which it is fulfilled. The terminal organs appear to be ganglia, standing in relation to the motor apparatus.

The nature of the disorders may be either to increase or diminish the proper function, as in the nervous system elsewhere.

The range of these nerves comprises four systems, in which the restraining action has at present been clearly shown by physiology. They are:—

1. The restraining system of the heart movements (cardiac, or that of Weber and Budge).
2. The restraining system of the respiratory movements (respiratory, or Rosenthal's).
3. The restraining system of the intestinal movements (peristaltic, or Pflüger's).
4. The restraining system of the reflex movements (reflex, or Setschenow's).

Corresponding to these are restraint neuroses of the heart, the respiration, the peristaltic movements, and of reflex action.

1. The restraint neuroses of the heart.

Their physiological basis is the experiment of Weber and Budge, showing that irritation of one or both vagi diminishes or wholly arrests the heart's action.

In pathological conditions a restraint producing irritation of the vagus can be excited at many points, although a direct irritation is uncommon, and an indirect or reflex one much more frequent. The direct irritation is seldom excited in the trunk of the nerve, or in its centre in the medulla oblongata, more frequently in the peripheral ramifications of the cardiac branches in the heart itself. In central irritation the violent further symptoms that are produced obscure the characteristic signs of vagus irritation. The same occurs also in the case of the so-called heart poisons, such as the salts of gallic acid, cyanide of potassium, and others; and in complicated brain disorder, such as concussion and tubercular meningitis. Neuroses of the vagus

form a part of the diseases comprised under the vague term, "angina pectoris."

The physiological type of a reflex heart neurosis is furnished by the well-known crushing blow experiment of Goltz. In this the irritation proceeds from the sensory nerves of the abdominal organs. Pathological cases that admit of the same explanation are sudden deaths from concussion of the abdomen, or from the passage of a catheter. To the same category belong many cases of angina pectoris due to irritation of abdominal organs, and attended by lowering of the heart's action, anguish, syncope, cold pale skin, and fallen countenance. So also do the effects of wounds of the intestine, many cases of (especially toxic) gastritis, of nervous gastralgia with lowering of the circulation, of stoppage or intussusception of the bowels with similar symptoms, of colic from biliary or renal calculi, and of peritonitis. The slow pulse of lead colic also depends upon vagus irritation. In all such cases death ultimately depends upon paralysis of the abdominal vascular system, as Goltz, in his blow experiment, has clearly proved. Mention should further be made of certain forms of nervous shock to the heart, with diminution of its functional activity from some abnormal irritation of the genitals, as in onanism and hysteria.

2. The restraint neuroses of the respiration.

The physiological facts that underlie these disorders are not so free from doubt as those that relate to the heart; but they are, nevertheless, certain enough. They rest upon the experiments of Rosenthal, which teach that slight irritation of the superior laryngeal nerve diminishes the frequency of the inspirations; that stronger irritation entirely stops them, with relaxed diaphragm and closure of the rima glottidis; and that the strongest irritation produces contraction of the expiratory muscles. In this way cough is occasioned, and, in the higher degrees of irritation, spasmodic cough.

Rosenthal considers the superior laryngeal to be a restraining nerve for the inspiratory movements of the diaphragm. Its excitation is not centrifugal, like that of others, but centripetal; and its restraining centre is certainly not, like theirs, in the peripheral organ, but in the medulla oblongata, so that an analogous direction of action is maintained.

Pathological conditions dependent upon restraint neuroses of the laryngeal nerve are numerous. Prominent among them are the spasmodic coughs, both the hysterical (a pure neurosis) and whooping-cough, which the authors hold to be an infectious neurosis. Again, there are the attacks of cough brought on by foreign bodies and by material changes in the air-passages.

3. The restraint neuroses of the intestinal canal.

These have their physiological analogues in the stoppage of the peristaltic movement, and the relaxed state of the intestinal walls from irritation of the splanchnic nerves, as first observed by Pflüger, and since confirmed by others. The restraining irritation is probably conveyed to the plexus that is placed between the muscular layers of the intestine, and that supplies the intestine with motor fibres. The characteristic of neuroses of the splanchnic nerves is the diminution or arrest of peristaltic action, producing retarded evacuation or complete stoppage of

the bowel. These symptoms, however, correspond to those of paralysis of the motor nerves. It is, therefore, difficult in concrete cases to decide which of the two causes is in operation. Very often there is a combination of both.

An example of irritation of the splanchnic nerves is furnished by the typical symptoms of lead colic, which are pain and obstinate constipation. The first may be very well attributed, in part at least, to irritation of the splanchnic nerves, since they contain sensory fibres. This irritation may itself strengthen the restraining influence by reflex excitation. The active character and the source in irritation of the costiveness are shown by the familiar therapeutic action of antispasmodics. It would certainly have the same character if caused, as some assume, by spasm of the intestinal muscles; but in such case it could only be reflex, and excited through the attacks of colic, and it would probably only occur periodically, and not occasion constipation of so obstinate a kind.

Besides the attacks of pain, and the relief afforded by antispasmodics, the presence of irritation of the splanchnic nerves is further denoted by a similar action upon other restraining nerves, especially the vagus; from which, in half the cases of lead colic, we find marked retardation of the pulse. The distension of the abdomen, again, indicates paralysis of the intestine. The reputed spasmodic contractions are probably only consequences of the pains, and are partly only apparent, and only affect the large intestine, upon which the splanchnic nerves appear to have no restraining influence. They may also be produced secondarily through the irritation of the mechanically-distended bowel, as we see in cases of internal obstruction.

It is probable that the psychical influences that modify the intestinal movements (as in hysteric meteorismus) follow the track of the splanchnic nerves, although their centripetal course, beyond the thoracic sympathetic ganglia to the cerebro-spinal centre, is not certainly known.

4. The restraint neuroses of reflex action.

The restraining action of the will upon reflex movements, and their promotion by decapitation, have long been known. Setschenow was, however, the first to determine experimentally the reflex restraining centre in the brain of a frog, in the corpora quadrigemina, and optic lobe. An analogous office is in the highest degree probable in mankind. The diseases of this restraining apparatus may serve to explain many spasmodic conditions—such as epilepsy, chorea, tetanus, and paralysis agitans. According to Malkiewicz, we may consider the spasms produced by poisoning with strychnia, alcohol, and opium, to be results of paralysis of the reflex restraining centre, upon which all these substances exert a decidedly paralysing influence.

ART. 41.—*On Exercise in Hysteria.*

By Mr. F. C. SKEY, F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

(*Medical Times and Gazette*, September 22, 1866.)

On this subject Mr. Skey remarks:—

“Such exercise should be active—neither strolling nor sauntering out of doors, ‘to take the air,’ as ladies term it, nor gardening, nor lounging about—but adopting a good brisk walk, at a pace of at least three miles an hour, *ever stopping short of fatigue*. People will often tell you they ‘take plenty of exercise about the house, and they are on their legs during many hours of the day.’ This is fatigue without exercise. What we want for health is exercise without fatigue, for fatigue is exhaustion, and it is to be obtained only on the terms I have mentioned. I do believe there are many maladies, or at least many forms of indisposition, which, with a strong will, may be walked away, provided the exercise be taken systematically, and rendered a prominent feature in the daily treatment. The distance walked should be increased daily, and a claim made on increasing strength for increasing exertion. I doubt whether horse exercise, however agreeable or however stimulating both to mind and body, is equal in sanitary value to exercise on foot. In the case of horse exercise the muscular exertion to an experienced rider, male or female, is very slight, and though the distance compassed may be great, the muscular exercise, so far as it is an important element of treatment, falls short of the requirement of health. That the effort is comparatively not great is proved by the long distances ridden, and the number of hours during which a delicate girl is seated on her saddle. The general concussion or shaking of the muscular frame incidental to this exercise in an unpractised rider subsides on its frequent repetition, and when the rider becomes familiar with the action of the horse so slight an effort is requisite to maintain the equipoise of the person in motion, and so entirely do the movements of the rider respond to those of the animal ridden, that the muscular effort amounts to almost nothing. Horse exercise, therefore, cannot strengthen the muscles, because it does not sufficiently exercise them. It is an agreeable and useful recreation, but I suspect its influence as a source of health acts more beneficially on the mind than on the body. I do not wish to undervalue exercise on horseback; I only desire to meet the too general belief that horse exercise can supersede exercise on foot as a means to restore health.”

ART. 42.—*On Hysteric Affections of Joints.*

By MR. F. C. SKEY, F.R.S., Consulting Surgeon to
St. Bartholomew's Hospital.

(*Medical Times and Gazette*, September 22, 1866.)

“With respect to hysteric affections of joints, knee cases, &c.,” Mr. Skey says, “they are in truth as common as Sir B. Brodie has declared them to be, and I thoroughly corroborate all he has said on the subject of this most important and interesting disease. Three-fourths of all knee cases in the upper classes of society, says this great authority, are not cases of inflammation, though they appear so. There is no organic disease whatever in the joint. They are cases of local pain, originating in impaired health. They are not amenable to treatment for inflammation and its consequences. Your liability to an error in diagnosis is just in proportion to the supposed infrequency of local nervous, as compared with vascular, derangement. The knee is by far the most frequent seat of these affections, and the cases are found among young women not in the lower class of life—but even this class is not exempt. You will find, on the occasion of your first visit, the patient walking lame. This lameness has existed for several days, probably weeks, before attention has been attracted to it, and has come on very gradually. The joint is stiff—not that it won't bend, but the movement is painful. There may be some increased heat in the joint when compared with that of the opposite limb, but not much in degree. The knee is slightly swollen. If you see the case after treatment has commenced—*i.e.*, after the repeated application of leeches, blisters, and tincture of iodine (the almost universal agent in difficulty)—the swelling will be palpable, and the outline of the joint has undergone a change. As the case progresses, the lameness increases, but the aspect of the joint remains as in the first stage—neither the swelling nor the heat increasing in the same proportion. In this condition the limb may remain for months, or even for years, subject to the same treatment, without improvement. One feature in this case ought to have struck you as worthy of notice—*viz.*, that so many months have passed without organic change; the joint is neither stiffer, larger, nor hotter than it was in the early stage of the treatment. I say it ought to have struck you. Perhaps it has not. The aspect of this lady is that of unhealth. She has become pale, partly from depletion, partly from loss of exercise. Her pulse is weak, her appetite bad, and catamenia, as a rule, defective. You fear to give tonics and alcohol, lest you aggravate the supposed local inflammation.

“Having exhausted the negatives in treatment, you now venture on an onward step, and you give bitter infusions, gentian, cascarilla, with ammonia and ether. But you are still behind the necessities of the case; you have adopted from the beginning a false diagnosis, and the difficulty is how to get back to the right groove. There is only one course: begin afresh, and treat your case on a different principle; convince yourself that nerves may go wrong as well as arteries and capillaries,

and as you treat excessive action, rightly or wrongly, in the blood-vessels by local depletion, so apply such remedies as check excessive action of nerves in the form of opium, belladonna, chloroform, &c. Build up the health by increasing the force of the circulation. The agents are a thoroughly nutritious diet, wine frequently in small quantities, tincture of bark, iron, fresh sea air, change of locality and associations, agreeable mental occupation. Assure your patient she has no real disease, but the semblance only. Leave the functions of the alimentary canal to take care of themselves. The constipation incidental to a low, innutritious diet and an inactive life will subside under the influence of a nutritious one; improved health will restore its functions. There is no real harm in a day's constipation; it is sometimes a good. At all events leave the bowels alone. With regard to the joint, rub in some blue ointment and extract of opium, in the proportion of one-third of the latter, and roll it firmly with a flannel bandage. Encourage moderate daily exercise on a level ground, on a carpet, or on a lawn. If the case is chronic, don't be disappointed if the progress be yet protracted to weeks. The pain and the stiffness may subside very slowly by virtue of their long possession by the joint; but you are in the right path, and rely upon it your patient's recovery will justify the sound principle of your treatment."

ART. 43.—*A Case of Tetanoid Convulsions overcome for a time by Application of Ice to the Spine.*

By Dr. J. W. OGLE, Assistant Physician and Lecturer on Medical Pathology, St. George's Hospital.

(*Edinburgh Medical Journal*, July, 1866.)

The patient, Henry F., aged two years, was a stout, well-made child, of a family free from consumption or other special taint, and had cut fourteen teeth. He had been ailing, with want of appetite and dulness of manner, for eight or ten days, but had not complained of any pain in any part. On 15th of February he had a powder given to him of some kind or other, obtained at the druggist's. On the following night he slept well, but about nine A.M. on the 16th he began to be attacked with convulsions, and about eleven o'clock he was brought to the hospital in a state of general convulsions, but affected chiefly on the right side of the body. At this time he was placed in a hot bath. Dr. Ogle saw him at half-past twelve o'clock, when he was still convulsed; but then it was chiefly the left side that was affected—the left arm, hand, and leg, and the muscles of the face being violently convulsed, in a clonic manner. The eyelids were widely separated, and the eyeballs quite fixed, and, to a slight degree, rolling from side to side. The pupils were larger than natural, but of equal size and unaffected by light. The tongue, which could not well be seen owing to fixing of the jaws, appeared to be clean. On touching the surfaces of the eyes or edges of their lids, but little reflex action could be produced. The surface of the body was much above the natural temperature, and the colour of the face was slightly, but manifestly, livid. The respiration was slow, and

attended by a degree of moaning. Dr. Ogle ordered an enema with castor-oil and turpentine to be at once given, and the gums to be examined for the purpose of seeing if they required lancing. It was, however, found impossible to open the mouth. He then ordered an ice-bag to be applied and kept in close contact with the back the entire length of the spine. At two o'clock the convulsions were much the same, the right side being now mainly affected, the hands being clenched, and the teeth firmly closed. The surface was warm; the head was frequently rolled from side to side on the pillow. The ice application was continued, and gradually the convulsions abated, and the child was considered by the apothecary well enough to be allowed to go home in the evening. The gums, &c., were found to be not swollen.

On the following day, as he was evidently not so well, the child was again brought to the hospital, under Dr. Pitman's care, and grey powder was given at bedtime. Antimony and salines were also given. On the day after (the 18th) one convulsive seizure took place, affecting both sides of the body, and then the child appeared to be better, no feverish symptoms remaining. It was only noticed that the child had somewhat of a wild, rather staring expression of countenance, the pupils being rather dilated. Nothing further occurred until the morning of the 19th, when convulsions recurred, in which the child died.

On post-mortem examination, the brain was found much and universally congested, and the grey substance dark in colour. The pia mater was universally injected, and many miliary scrofulous deposits were found attached to its inner surface, especially in the great longitudinal fissure, and on the velum interpositum, and on the upper surface of the cerebellum. A small quantity of recent fibrine existed beneath the arachnoid at the inner side of each Sylvian fissure, the neighbourhood of the optic commissure and pons Varolii being almost free. The lungs contained about five or six more miliary scrofulous deposits; the heart, liver, and kidneys were natural; the spleen also contained a few scrofulous deposits.

In commenting upon this case, Dr. Ogle drew attention to several points appearing to deserve notice. In the first place, the *spasm* which existed was such that, at the outset, seemed to have quite the character of tetanus, or of the convulsion produced by strychnia; and suspicion of the latter was the more strong at first when it was made known that the child had had some kind of powder exhibited previous to the setting in of convulsions; but as it was found on investigation that the convulsions did not occur until several hours afterwards, this suspicion fell to the ground, especially when it proved that the spasm was, to a considerable extent, *unilateral*. This unilateral character of the spasm was also most interesting, considering that, as it ultimately proved, the spasm was connected with a general state of meningitis. At first, owing to the absence of fever, the suddenness of attack, &c., Dr. Ogle thought the case was one of mere congestion of the nervous centres; and indeed was even, after all, inclined to suppose that the early convulsions may have coincided with a simple state of congestion prior to the effusion or production of lymph, which, as it was subsequently ascertained, occurred within the cranium. If so, of course the formation of this lymph (considering how speedily death took place after the first symptoms set in)

must have been very rapid; but Dr. Ogle pointed out that a very few hours may suffice for the formation of such lymph and so-termed exudation. The substantial relief from the ice application was not a little interesting, and speculation upon the probability of a greater and more permanent benefit from a longer application of the ice could not be resisted. A noticeable symptom was the *lividity* of the face—one evidently connected with some pulmonary congestion. This symptom might have been thought to be the result of interference with the movements of the chest-walls by reason of spasm of the thoracic muscles; but as it was associated with a *slowness*, and at the same time *regularity*, of respiration, the juster inference was, that it was the result of some intracranial disturbance. The *dilated* state of the pupils Dr. Ogle was more inclined to connect with the general strumous condition of the body than with the cerebral effusion, &c.

ART. 44.—*On the Relation of Chorea to Rheumatism.*

By M. ROGER, Physician to the Hospital for Infants, Paris.

(*Journal of Practical Medicine and Surgery*, 1866.)

Chorea, rheumatism, and heart affections, M. Roger observes in a clinical lecture on the pathology of chorea, belong more especially to the second period of childhood. These diseases seldom begin before the third or fourth year of life, and culminate between the ages of seven and fourteen. The latter age coinciding with puberty, Bouteille conceived that this period of transition of the system exercised an active influence on the development of the symptoms. This, however, is far from being demonstrated, and M. Roger contends that the occurrence of chorea at this stage of adolescence is merely the result of the frequent coincidence of rheumatism at the same age.

As a proof of the neurotic nature of chorea, writers have adduced the fact that it is more commonly observed in females; that in both sexes the disease is often the result of irritation; that it is not hereditary in the same degree as rheumatism; and that it often is induced by fright or sudden mental emotion, causes incapable of giving rise to rheumatism. The facts brought forward in support of this line of argument are doubtless striking and singular, but they are not in accordance with truth. If these extraordinary cases be carefully sifted, rheumatism will be found to have preceded the alleged nervous attack. A little girl was recently brought into hospital for the treatment of choreic symptoms, said to have originated in a sudden fright. On inquiry it was distinctly ascertained that the chorea had been preceded at a week's interval by undoubted rheumatism. With regard to chorea consequent on imitation, either the symptoms are feigned, or are a secondary manifestation of a rheumatic affection. Neither M. Blache nor M. Sée nor M. Roger has ever met with a genuine case of chorea caused by imitation. Bricheateau, it is true, published nine instances which he referred to this influence; but six of the subjects were affected with hysteria, a morbid condition in which the patient's truthfulness is

deserving of very slender confidence, and in the three remaining cases M. Roger inclines to the belief that M. Bricheteau was unaccountably deluded.

Mental agency should, therefore, be erased from the list of the causes of chorea. Indeed, the whole structure of the etiology of the disease should be raised to the ground, leaving but one single determining influence—viz., rheumatism.

Daily experience confirms in the most unmistakable manner the truth of the surmise, that the co-existence of chorea and rheumatism is not a mere chance coincidence, but that both affections are connected with each other in the closest manner by a reciprocal relation of cause and effect, and by some intimate and unvarying affinity. The parents of choreic children, when questioned on the subject, acknowledge, in one-half of the cases, that rheumatism preceded the spasmodic manifestation. If rheumatism has not already occurred, it will assuredly soon follow, the interval seldom exceeding a month. Chorea sometimes supervenes at the conclusion of, or during convalescence from, rheumatic fever, and sometimes in the period of status of the latter.

The identity of chorea with rheumatism may be inferred from another circumstance—namely, the similarity of the complications in both affections. Cerebral chorea is not less frequent than cerebral rheumatism, and the very common coincidence of heart disease in one-fourth, at least, of the cases of chorea, visibly demonstrates the true nature of the latter.

In some instances an attack of chorea alternates with an attack of rheumatism, as if the morbid condition merely changed its form.

From these motives, which are only the leading arguments which might be brought forward, we may, M. Roger thinks, hold ourselves justified in asserting that chorea is almost invariably identical in nature with rheumatism, and hence that it must, like rheumatism, exercise an important influence in the production of diseases of the heart.

ART. 45.—*On the Diagnosis of Symptomatic and Essential Paralysis of the Sixth Pair of Nerves by means of the Ophthalmoscope.*

By M. BOUCHUT.

(*L'Union Médicale*, July 3, 1866; *Medical Press and Circular*, September 12, 1866.)

M. Bouchut reports several cases of paralysis of the sixth pair of nerves, with especial reference to the ophthalmoscopic appearances observed in them; and he states that henceforth, in paralysis of the external motor nerve of the eye, we must add the use of the ophthalmoscope to the study of the antecedents and the history, as an essential aid in the detection of the disease. For by its means we shall often be enabled to detect the organic nature of the lesion by the discovery of a morbid condition of the fundus oculi. The paralysis may be *spontaneous* (Valleix), *rheumatismal*, produced by cold (Badin d'Hurtebise); it may

depend on *constitutional syphilis* (Bégran); *albuminuria* (Landouzy); *diphtheria* or *chlorosis*, plumbism, or a *fault of accommodation*, or *optic neuritis*, produced by excessive use of the eyes. It may also be referable to *chronic meningitis*, produced by a wound over the eyebrow, or by a fall on the head, or finally it may be due to localized chronic encephalitis, or tumour of the brain.

In many cases the evidence furnished by the patients, and the symptoms which they present, suffice for a diagnosis, but in those cases in which there is a suspicion that the cause of the disease is seated in the nerves or nervous centres, the ophthalmoscope alone can determine the question with certainty. In short, if there exists a serous or granular infiltration of the papilla or retina, in consequence of either general or partial congestion, venous thrombosis, or retinal hæmorrhage, we may be satisfied that there has been either *optic neuritis*, or localized *chronic encephalitis*, *chronic meningitis*, or *tumour of the brain*.

These lesions of the eye do not, it is true, always indicate the nature of the cerebral affection; but what is of chief importance is, that their demonstration, nevertheless, proves the organic nature of the paralysis, and this is undoubtedly a great advance.

ART. 46.—*On Sclerosis of the Brain and Spinal Marrow.*

By Dr. W. ZENKER.

(*Zeits. für Rationelle Medicin and Brit. and For. Med.-Chir. Review.*)

W. Zenker, of Gottingen, contributes observations made by him during some months past on this subject. The microscopical investigation of sclerosed spots in the brain, made in a fresh condition, gave the following results:—Connective tissue-fibre existed in abundance, containing many interspersed nuclei, whilst no trace of brain-elements and nerve-fibres could be discovered. Moreover, amylaceous corpuscles, recognised as such by their reaction with iodine and concentrated sulphuric acid, were found scattered in great abundance and lying about in heaps in the field of vision, in size from that of a connective tissue-nucleus to that of a frog's blood-corpuscle, as round bright bodies, in which here and there a nucleus and also a concentric arrangement were distinguishable. As it seemed to be necessary to make a further investigation with finer instruments, some large sclerosed spots of the white substance of the cerebral hemisphere were hardened—a part in most highly-rectified alcohol, and another part in a solution of two parts chromic acid to 1000 of water. After fourteen days the further examination was made, and the following results obtained:—

1. In the chromic acid preparation, sufficiently fine sections, made with a razor, showed a fine connective tissue, consisting of fibres. These appeared to be in part arranged as a network, in the meshes of which appeared the form-elements to be mentioned later. In thicker sections there was found only an apparently finely-granulated mass, in which it appeared that many fibres were cut obliquely or across. This partly net-shaped arrangement of the fibres was not probably an arti-

ficial production by the coagulating influence of the chromic acid solution upon an albuminous fluid, because, in certain spots, the fibres united into thick bundles passed away parallel among each other. The same fibres, but less easily, could be shown in the alcohol preparations. After the addition of acetic acid the fibrous arrangement disappeared altogether.

2. In the alcohol preparations, but not in those made with chromic acid, were found those larger and smaller concentric corpuscles which, in a fresh condition, the iodine sulphuric acid reaction exhibited.

3. In the chromic acid preparation there were found, moreover, in the meshes in question, very numerous oval, somewhat flattened, or egg-shaped and incurved nuclei. There could not positively be shown around the nuclei, cell-processes and cell-membrane, although in carmine preparations the appearance of spindle-shaped, star-shaped, or anastomosing cells was often very evident. This was sometimes produced by the nuclei lying imbedded in the meshes of the fibrous bands crossing each other. In no way could anything but nuclei be isolated. Fine sections of the alcohol preparation, moreover, were treated with the ammoniacal carmine solution, then washed out with a saturated solution of acetic acid in distilled water and concentrated glycerine; then, again, the covering glass being frequently lifted, it was washed with water, and finally the preparation examined in a glycerine solution. By this method the nuclei described were found to be dyed red.

4. Also, by the treatment just mentioned, the capillary vessels appeared as distinct strings furnished with very numerous nuclei in their coats. As to the capillary vessels themselves, many of them showed considerable fatty degeneration of their coats, which was very beautifully seen by treating the alcohol or chromic acid preparations with diluted soda solution.

5. Independently of the sclerosed parts the rest of the brain showed no essential change. Ganglia cells, however, were not unfrequently met with in the vicinity of the sclerosed spots. These were decidedly fatty degenerations, for they were almost completely untransparent, and filled with closely-packed fatty granules; so also were their processes.

From all this taken together it results that, viewed microscopically, it was a pathological new formation of a tolerably distinct fibrous connective tissue with overgrowth of its nuclei, and of the nuclei of the capillary vessels with a probably secondary fatty degeneration of the coats of the latter. The ganglia-cells may then have been changed by pressure of the callous connective tissue.

The microscopical examination of the spinal cord, preserved in alcohol, gave the following results:—In the cervical region, sections of the posterior columns made with a razor, treated with an acetic acid solution, showed the same characteristics as did the sclerosed spots of the brain—a network of fibrous bands (here even more pronounced than in the brain); abundantly interspersed nuclei; concentric corpuscles; fatty degenerate capillaries—and in the same proportions as in the brain.

Cross sections of the lateral columns showed neither connective tissue-fibre with nuclei, nor fatty degenerate capillaries, but, on the other hand, a large number of normal nerve-fibres, in some parts single concentric, and here and there brown stellate corpuscles resembling pigment cells.

ART. 47.—*On late Rigidity in Hemiplegia.*

By M. CH. BOUCHARD.

(Archives Générales de Médecine, Septembre, 1866.)

In a memoir on secondary degeneration of the spinal marrow, M. Bouchard maintains that it is an error to attribute late rigidity to chronic irritation of the brain arising from the cicatrix of the primitive lesion, or to the progressive course of supposed encephalitis. The cause of the rigidity, he believes, is to be sought in the spinal cord. It cannot be assigned to fatty, granular degeneration of the nerve tubes, a change which is not followed by any characteristic symptom. Moreover, prior to the rigidity commencing, the nerve tubes injured in the encephalon have been destroyed in their entire length. But the nerve tubes of encephalic origin are mingled in the spinal marrow with other tubes which arise in the grey matter of the cord. These medullary tubes are buried in the midst of a tissue which, a considerable time after the occurrence of apoplectic lesions of the brain, is the seat of a very abundant conjunctive proliferation. To the irritation of the medullary tubes by this neoplasm—that is, to this secondary sclerosis—M. Bouchard attributes the late rigidity in hemiplegia.

ART. 48.—*Cure of Masturbation by Amputation of the Clitoris and Nymphæ.*

By Professor G. BRAUN.

(Wien. Med. Wochenschr., 1866; Schmidt's Jahrbücher, 1866.)

Professor Braun records another case of the cure of masturbation and its consequences by this operation.

The patient was an unmarried woman of the better class, twenty-four years old, who had been abandoned to masturbation in an excessive degree since the age of fifteen years, and who had evidently suffered from it both in physique and in intellect. She had been four or five years under medical treatment. Upon examination the clitoris appeared normal, but became erect upon the slightest touch, so that it could be felt as an elevation the size of a goose-quill. Contact with the clitoris excited rhythmical contractions of the abdominal muscles, nates, and labia, from which, in about twenty seconds, an albuminous fluid flowed over the posterior commissure. The præputium clitoridis was lax and deeply pigmented; the nymphæ projected an inch beyond the labia; the hymen was uninjured and very extensile. The uterus appeared normal.

The author performed amputation of the clitoris and nymphæ with the galvano-caustic loop. The first application left the stump of the clitoris still projecting, and the apparatus was applied a second time to remove

this completely. The operation lasted scarcely a minute, and was neither attended nor followed by bleeding or any other accident. The parts removed were in a seemingly normal condition.

The healing progressed favourably. The eschar fell on the seventh day, and there was some difficulty in preventing union of the opposed surfaces. After three weeks there was a smooth cicatrix. The base of the clitoris could still be felt, but contact with it produced no irritation nor reflex movements. The sleep was already much more tranquil and undisturbed, and the masturbation was no more practised. At the same time the general bodily condition was better, and an increasing interest was taken in outward affairs.

The author advises amputation of the clitoris and nymphæ in cases of habitual onanism in women, when not only physical but also psychological disorders are produced, and when ordinary remedies have been used without benefit. He thinks the galvano-caustic loop the best method of operating, since its use is not attended by hæmorrhage.

ART. 49.—*On a Case of Inability to Talk, to Write, or to Read Correctly after Convulsive Attacks—subsequently Choreic Movements.*

By Dr. HUGHLINGS JACKSON, Assistant Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital.

(*British Medical Journal*, 1866.)

This case derives its chief interest from the fact that the patient, who, though a hospital patient, was well educated, had lost a good situation because, after certain convulsive attacks, he became unable to spell, and was thus unable to perform the duties of clerk in an important Government office.

There was no certain evidence to point to the side of the brain diseased. Besides the convulsive seizures, the patient has had attacks of a very curious sort. "They have," Dr. Hughlings Jackson says, "resemblance to choreal movements; and thus to my thinking they have much importance, as showing, with the other defects I am about to mention, the continuity of movement, actions, talking, and those conditions of nerve-tissue which do not necessarily result in outward movement; *e.g.*, subjective talking. It is important to add, that this patient had had rheumatic fever thirteen years ago. I think it most probable that the nutrition of parts of his hemispheres has been interfered with by plugging up small vessels."

After referring to the researches of Banks, Russell, and Sanders, Dr. Hughlings Jackson continues:—

"I met my patient in the street a few weeks after my visit to him just mentioned. He was then, to superficial appearance, as well as ever. I observed that he spoke quite well, and this throughout rather a long conversation. If he had made the slightest mistake of any sort, I

should have caught it at once. I congratulated him upon being able to speak well again. He replied, however, that he was often at a loss for a word; and his father told me that his son frequently made mistakes in names. On my remarking that I had not detected any defect of speech, the patient said that his speech was imperfect most 'when anything came on him suddenly,' or when he was not thinking particularly of what he was saying. His greatest trouble, however, was in writing. He had no difficulty in penmanship; on the contrary, it was beautiful. His trouble was that he could not readily find the proper words, and those he wrote he often spelled incorrectly. He showed me something he had just written; namely, words on a plan. For 'box' he had written 'gox;' for 'silver,' 'cilver;' and again I saw that, after crossing out this mistake, he had written 'silves.' I was extremely interested in his mistakes, for there was mind enough to give them relation to proper speaking and writing. I asked him to collect for me all the mistakes he had made in writing, and in a week he brought me several letters. He said he could generally manage to write a tolerably correct letter if he made a copy first and then looked over it.

"The words in the square brackets are corrections written by the patient; those in the curved brackets are written by myself.

"I glad to say that I am going on all right, and I home [hope] to continue to do so. I galy [daly] take a long walk, and do not find the configue [fatigue] as I formerly did. I am aglie agissue agligere (obliged?) to stop and think what ("what" is crossed out) how spell (crossed out) the wors [words] are spelt. I can ver ger generly (altered to generally) go on verly well in may makeing the second copy."

"He brought me, also, a bundle of letters written before he was ill; I could find no mistakes of any kind in them."

Dr. Hughlings Jackson then gives the second copy referred to, but it also contains words spelled wrongly. After giving other letters, Dr. Hughlings Jackson gives an extract copied from a book, in which extract there are scarcely any mistakes. It is as follows:—

"The place of our retreat was in a little neighbourhood consisting of farmers who tilled their our [own] grounds, and were equal strangers to opulence and poverty. As they had almost all the convenience [s] of life within themselves, they seldom visited towns or cities in search of superfluities. Remote from the polite, they still retain [ed] the primæval simplicity of manners; and frugal by habit, they scarce knew that temperance was a virtue. They wrought with cheerfulness on days of labour."

"The patient then read aloud the passage just given. He read very slowly, and made, as nearly as I could estimate from the sound, the following mistakes, many of which he corrected. Red, round, hand, for neighbourhood; standers for strangers; opulus for opulence; possery, popery, for poverty; seppertition, sepperist, sepperit, sepperistis—abortive efforts to say superfluities (he could not say it after me until I said it very carefully and slowly for him); remake, remoke, remoke, for remote; polites for polite; primavel for primæval; mim- plicity for simplicity; menners for manners; fruel for frugal; themperance for temperance; cheerlessness for cheerfulness; lady for labour."

"It may be that such mistakes as 'polites,' 'fruel,' are due to carelessness; but he pronounced these mistakes very clearly, and corrected them sometimes. Nor were the mistakes due to a permanent difficulty in articulation. He could say any word I asked him to say. He could repeat without the slightest slip the following difficult lines.

'Around the rugged rocks the ragged rascal ran the rural race.'
'Up a high hill he heaved a huge round stone.'

"The following was dictated from an article in the *Saturday Review*. His mistakes are in brackets.

"The man [mand] whose [woos] mind [minds] is entirely taken up [out] with small [sall] details [detales], fancies [sances] he has a right to sneer [seen] at every one gifted [gisted] with less [lest] minute knowledge [nowledge]. Because [Begase, and again Begause] he can [gan] grease [crease] the wheels [weels] and tighten [bighten] the screws [schrees] of machinery [masheenary] he fancies [sances] himself an authority on the laws [laus] of motion [mosien].'

"I then asked him to spell the word whose, aspirating it strongly. He wrote 'hose.' Small, he spelled 'sall,' as in the text; but quickly remarked, 'No, that's not it; that's sall.' He then hesitatingly spelled it rightly. Sneer he spelled snier; because, begause; and laws, lass. I said no. He then spelled 'lause.' I again said no. He said interrogatively, 'There is a laws spelled l a s s?' I said, 'That is lass, not laws.' He replied, 'So it is;' but still could not spell the word correctly.

"The reader will observe how much worse the spelling is in the preceding specimen than in what the patient copied from the test-types. Whilst he was copying, I noticed that he kept referring to the original for nearly every letter. He transferred each particle quickly, so that it lost nothing in carriage. He did not trust it to his memory for a moment. To use a simile, it passed from his eye to his fingers without any adulteration from his own damaged organization. The patient brought me the following since the previous part of this paper appeared. I give it in his spelling:—

"The great fault in me sempt (crossed out) seams to be that I cannot speel when writing, if fact (crossed out in pencil) at some times I cannot at first recollect how to put down the Letter L. I have ofter been bothered as to how make the note* (letter) until I but it down in my memory by spelling my own name. (His name begins with the letter L.) Sometimes I am bothered to recollect various letters, and then I run the A B C in my head until I cum (crossed out in pencil) come up to the note (letter?) I want, and then I can bring it out to m'—

"My patient tells me that he frequently cannot write a letter until 'I have got it before my eye.' When he said this, he put his hand before

* "The word note was crossed out, and the word letter substituted; but I observed that throughout our conversation on the statement he had written out, he used the word note for letter. Neither his father nor myself could get him out of this use of the word. He stared at our objections vacantly."

him. He could make the motions for using a pen; but he had lost the power of reproducing completely the impulses for the particular actions he had learned for the writing of particular words, and had, as it were, to submit to a new, although a transitory, education when copying.* There seems a difficulty here in reproducing images in his various sense apparatus and their cerebral connexions.

"It must be observed that these last-mentioned trials are rather specimens of what he could do by care and by attending to what he was doing, and not of what he did say when talking loosely. As he still said that he had made mistakes at home, I asked him to collect them for me. The following is the result:—

"Intending to say the following words:—I sayed, for case, clase; for sister, sisper; for stomach, spomach; for that, sthat; for never learnt, never lant, never tant; for to wear, to pear; for plate, s-s-s-plate; for three, th-th th-ree; for pig, prig; for bedstead, beckstead; for Emma, Enna; for go to bed, go to ded; for reid [read], leid; for coat, sloat; for turn his toes out, turn his nose out; for later in the day, laer in the day; for answer as a tung [tongue], answer as a tooth; for how is your tooth? how is your hand—your tongue—your feet? for mistake or two, mistalabal; for to such extent, to such an expemut; for going to wear, going to vell; for sticking plaster, picking plaster; for sight, fight; for blood, brod; for beat, bread; for going right, going rike; for you need not wet it, you need not wat it; for you have a little cold, you have a little clean; for the sun is at its height, the sun is at the moon; for you want some more meat to eat with them, you want some more meat to drink with them; for meat, bread; for knife, eggs; for wall, floor; for walk a mile, take a mile; for of meat, of mead; for not been my doing, not been ny doing; for nise [nice], nite; for tight, trite; for of a boy, of a dog; for alf [half] a sleap [sleep], alf a sleass; for travel, traverk; for he is nocked [knocked] up, he is locked up; for to school on Saturday, to school on Skatterdlay; for people coming from church, people coming from slearch; for one stair, one floor; for all the week, all the sweet; for you had better save them, you had better ceave them; for its all mussle, it all mujjle; for custard, tustard; for shows as bright, showes as shite; for table, tadle; for the fastest train, the largest train."

"I called on him one day, and asked him to spell several words. Some he spelled by ear. For plough he gave plow; for cough, coff; for dough, first dough, then dowe; for daily, daly; for generally, he began several times j, and when I told him it began by g, he could not proceed; for laugh he gave lauf (he pronounced the word something like this). For picturesque, at the first trial, picthureess; at the second

* Professor Bain says (*Fortnightly Review*, February 1st, 1866):—"It must be considered as almost beyond a doubt, that *the renewed feeling occupies the very same parts, and in the same manner as the original feeling, and in no other parts, nor in any other manner, that can be assigned.*" Again, he says:—"For every act of memory, every exercise of bodily aptitude, every habit, recollection, train of ideas, there is a specific grouping or co-ordination, of sensations and movements, by virtue of specific growths in the cell-junctions."

trial, picthuessk ; at the third trial, he said, 'No, that's not right,' and deliberately spelled 'esque.'"

ART. 50.—*Defects of Smell in Epileptiform Seizures, in Mental Affections, &c.*

By J. HUGHLINGS JACKSON, M.D., Assistant Physician to the Hospital for Epilepsy and Paralysis, and to the London Hospital.

(*The Lancet*, June 16, 1866.)

In a report of some remarks by Dr. Hughlings Jackson on defects of Smell in diseases of the Nervous System, the writer urges that these symptoms are quite as significant as, although less important than, loss or defect of sight. Before we quote from the present article, we may make an extract from one in the *Ophthalmic Review*, April, 1866, to which article we alluded in our last vol. p. 139. Dr. Hughlings Jackson writes:—

"Smell is not unfrequently lost when there is amaurosis, and I cannot but urge that inquiries should always be made as to defects of this sense when sight is affected. In one of the two cases of loss of smell in which I have had an autopsy, the disease has been of the hemisphere, at a distance from the optic thalamus ; but I have not yet had a satisfactory case completed by an autopsy.

"When smell is lost, the patient generally says that his taste is defective too. We must then be particular in stating whether our patient can tell the bitterness of quinine or the sweetness of sugar, when we find that he does not know the flavour of such things as oil of pepperment or of cinnamon when placed on his tongue.

* * * * *

"In all cases of amaurosis—certainly in all cases of amaurosis with hemiplegia or epileptiform seizures—we ought to investigate the conditions of the other special senses, with the object of obtaining positive and negative evidence as to the seat of damage, which does, or may, give rise to defect of one or more of them."

In the *Lancet*, Dr. Hughlings Jackson, after speaking of cases in which subjective sensations of smell occur with epileptiform seizures, and with symptoms of mental disorder, remarks:—

"Defects of smell and defects of mind may seem to be things which can have little to do with one another. But the olfactory bulb has, at the least, a geographical relation to a great part of what is believed to be an important division—the anterior lobe of the cerebrum—of the chief organ of intellectual life. This relation is quite as important in one way as that of the auditory nucleus to the centres for the chief functions of animal life is in another. Perhaps the mere geographical relationship of one olfactory bulb (possibly it might be better to say of the olfactory convolution) to the mass of the anterior lobe of the brain

may not strike most people—especially those who think that the brain is a double organ—as a fact sufficiently important to encourage us to spend much time in searching for evidence of any kind of relationship betwixt smell and intellect.

“In thinking, as physiologists, chiefly on the common relations of the various organs of sense (with their perceptive centres) to the hemisphere, we must, as physicians, pay equal attention to the wide differences in their geographical position. Although a much inferior sense, the physiological relationship of the centre for smell to the hemisphere is quite as significant as that of the centre for hearing, whilst its geographical relations are much more important. Still, clinical medicine shows that we must be very careful how we interpret the series of symptoms with which loss or defect of the special senses occurs, by reasoning either generally from the basis of a recognition of their relations as similar functional centres, or more narrowly from a consideration of the contiguity of their centres with other parts. Thus, in spite of the common relationship of the perceptive centres to the cerebrum, loss of hearing is rare, and loss of sight not unfrequent, in cases of disease of the hemisphere. Then as regards contiguity, the auditory nucleus is, Lockhart Clarke says, actually continuous with that of the vagus; and we know that experimental injury betwixt the origin of the auditory nerves (see particularly Roberts *On Urinary Diseases*) produces temporary glycosuria. Yet we hear nothing of disturbances of respiration nor of sugar in the urine in any cases of sudden deafness; possibly, however, the sugar is not sought for soon after the deafness comes on.”

Now Dr. Hughlings Jackson thinks that, besides the physiological relation of the olfactory nervous system to the hemisphere—the nervous system of the nervous system,—and besides the geographical relations of the olfactory bulb and the anterior lobe as two masses of tissue, there is another relation which these two divisions of the nervous system may be considered to have to one another—viz., by their arterial supply. A single artery—the anterior cerebral—supplies part of the bulb, a great number of convolutions not only of the anterior lobe, but of a great part of the length of the inner surface of the hemisphere, and, what it is very important to bear in mind, the great commissure, the corpus callosum.

* * * * *

Dr. Hughlings Jackson states by saying that his experience of cases of insanity is so limited that he has much difficulty in giving the proper value to the facts he has already collected, especially as they were, superficially at least, in apparent contradiction to one another. He is anxious to learn from psychologists what has been done in the matter, and especially if defects or loss of smell are met with in puerperal mania, which form of mania recurs under the same conditions as occasionally give rise to hemiplegia and chorea, both which diseases are due, the former sometimes, and as Dr. Hughlings Jackson thinks, the latter generally, to plugging of branches of the cerebral arteries.

ART. 51.—*The Form of Amaurosis occurring in Locomotor Ataxy and in Disease of the Hemisphere.*

By J. HUGHLINGS JACKSON, M.D., Assistant Physician to the Hospital for the Epileptic and Paralysed, and to the London Hospital.

(*Medical Times and Gazette*, September 1, 1866.)

In this paper Dr. Hughlings Jackson suggests that three forms of disease, or rather three sets of symptoms, should be considered in reference to sex, both as to physiological peculiarities, habits, and vices. These diseases are general paralysis, locomotor ataxy, and certain forms of amaurosis; the different phenomena depending, the author hints, on one condition of nerve tissue. Therefore, the object is to merge these entities under one general pathological condition. We shall, however, extract only what relates to locomotor ataxy, and chiefly those parts which refer to the ophthalmological aspect of this disease or set of symptoms.

"In considering the causes of atrophy of the optic nerves," Dr. Hughlings Jackson writes, "we ought to keep continually in mind that it is not always (1) the result of a gradual and slow process, but frequently (2) the remains of an acute one. If Mr. Wordsworth's researches [to which in reference to so-called tobacco amaurosis, the author particularly refers] had no greater merit than that of putting this prominently before us, they would have great value. As I have just remarked, there are at least two kinds of atrophy of the optic nerve, or at least two sets of ophthalmoscopic appearances. Perhaps, as a physician, I should speak more safely if I were to say that there are but two marked kinds in patients who come under our care for diseases of the nervous system *par excellence*. These two forms of atrophy are in general alike, but they present particular differences. There is one in which the disc simply whitens, and often changes slowly; and another in which its edge is ragged and irregular, or in which it gradually merges into the red fundus. There is often much pigment around it, and its arteries are generally very small and its veins comparatively large.

"I have traced the second kind of atrophy many times from the well-known condition called neuritis. (Swollen disc, arteries lost, veins large, and scattered effusions of blood.) This kind of atrophy is the one found in the amaurosis which occurs with hemiplegia, and with epileptiform convulsions (generally unilateral). It is the kind of amaurosis which follows disease of the hemisphere. Now, my experience of cases of amaurosis with locomotor ataxy is not large enough to enable me to speak with certainty of its exact ophthalmoscopic appearances. In the case to be alluded to, in reference to the pains in the legs, and in which, at the autopsy, there was found disease of the posterior columns of the cord, the discs were white, but well-margined, and their vessels of fair size. I will mention a third case in association, although it belongs to a different category; I mention it chiefly for contrast. A

patient consulted me for difficulty in walking and defect in sight. He had a swollen optic disc, the arteries were lost in the discs, and blood was effused about them. He could then, however, see fairly, and could read. When his eyes were shut, he could stand, and even walk, but had great difficulty in doing so. He afterwards became almost blind (the discs whitening), and then, although he retained great power in his legs, could not walk a step, nor, indeed, stand without assistance. I confess I thought this to be, if not a pure case of locomotor ataxy, at least one resembling it; but at the autopsy I found a tumour at the base of the brain, pressing on the left crus cerebri, left side of the pons and crus cerebelli. The corresponding lobe of the cerebellum was smaller. This case is a valuable one, in showing the use of the ophthalmoscope in diseases of the nervous system, and points strongly to the importance of making a precise record of the ophthalmoscopic appearance in this class of cases, even when the amaurosis is but a subordinate symptom.

"One of these forms of amaurosis might be called spinal, the other cerebral.

"Looking at the notes of the cases I had under my observation while clinical assistant at Moorfields, after a wider experience of diseases of the nervous system, I cannot help suggesting that it is more than probable that many of the cases of atrophy of the optic nerve in males which I saw at that hospital, especially those in which there were pains and numbness in the legs, were cases of commencing locomotor ataxy, or were loosely allied to that disease.

"It may be justly remarked that the term 'numbness and pains in the legs' is to the last degree vague. But in writing generally of many cases I must use a general expression. The pains, when particularly described in my notes, are recorded as 'gnawing,' 'shooting,' &c., such as any physician who has paid attention to progressive locomotor ataxy will recognise as being frequently met with in that disease. In taking notes of cases for scientific purposes, it is most important to get the patient to describe these subjective symptoms as carefully as he can. When they are faithfully described, it is of less importance how they are named, but to name them as 'rheumatic' without description would lead to error. The use of a loose *name* necessitates careful *descriptions* in actual cases. It will be observed that these pains are noted to have occurred in but one of the female patients. A few months ago I made a post-mortem examination on the body of a male patient who died amaurotic, and who during life had had symptoms of ataxy. This patient used to describe his pains very vividly. He said they were gnawing, and 'like a dog biting him suddenly.' I found disease of the posterior columns* in the dorsal region of the cord. This case shows, I think, the relationship of 'uncomplicated' amaurosis to locomotor ataxy. The patient's sight failed gradually, and was lost in two years. Twelve months later pains in the legs began, and it was several years after that before any notable difficulty in walking occurred. The optic discs in this patient were *well-margined*, and the vessels were of good size.

* The specimen has since been carefully examined by Lockhart Clarke. See *Lancet*, June 10, 1865.

"I repeat that in speaking of pains in the legs accompanying amaurosis, I mean pains of a particular character, viz., like those accompanying locomotor ataxy. Now, I by no means wish to assert that these pains with amaurosis should lead us to diagnose commencing progressive ataxy, but they are certainly often definite enough to command particular attention in any methodical investigation of amaurosis, and quite definite enough to be admitted to the class of symptoms to be investigated as regards sex, tobacco, &c. In diseases of which the real pathology is so much disputed as amaurosis and locomotor ataxy, we can do little more than group our cases from clinical symptoms. This is certainly not a desirable kind of order, but it is the only one, I submit, that is at present, practicable; and if not scientifically exact, it, at all events, will render what one has to say intelligible, and will render a better one possible. Whether, then, there be a real relation betwixt what we call uncomplicated amaurosis and locomotor ataxy, there is, at least, a clinical gradation in the symptoms from one to the other, case by case. And it is this gradation which shows, I submit, the scientific importance of these pains. They are a link betwixt 'uncomplicated amaurosis' and locomotor ataxy.

"Thus we have (1) amaurosis without pains in the legs; (2) amaurosis with pains in the legs only; (3) amaurosis with pains in the legs, and difficulty in co-ordinating the movements of the legs; (4) pains in the legs, and difficulty in co-ordinating the legs without amaurosis; (5) amaurosis without pains in the legs, and with difficulty of co-ordination. I could now put five patients in a row showing the above sets of symptoms. I speak of the kind of disc I saw in the patient who died with disease of the posterior columns. I have now under my care at the London Hospital a case which shows the connexion of amaurosis with pains in the legs, and as these pains are like those in locomotor ataxy, I may say the connexion of amaurosis with one, at least, of the symptoms of this disease. The patient, a man forty-five years of age, came to me for symptoms of debility and nervousness of no very definite character. Finding that he was blind, I took the opportunity of investigating the causes and relations of the amaurosis. His sight began slowly to fail ten years ago, and it was four years before he was absolutely blind as he now is. The optic discs are quite white, but they are well-margined, the arteries are small, and the veins also are rather small. This patient has no difficulty in walking, and finds his way from my room to the chair for the ophthalmoscope in the next room easily and quickly without any help. He had never had any difficulty of this kind except when he had had pains in his legs, and then only at the moment when they seized him. He had therefore never had the essential symptom of ataxy, but still the pains were sufficiently characteristic to show some relation betwixt the two diseases. He had had them for three years before the blindness began, and for seven years afterwards. They were severe shooting in both legs, coming on suddenly. He remarked spontaneously, 'I can compare them to nothing but a flash of lightning.' I may just add that he had not smoked much—never more than half an ounce a week—he had been temperate, and had not, he assured me, been addicted to sexual excesses, marital or otherwise."

ART. 52.—*Syphilitic Disease of the Arteries of the Brain, with Remarks on the Treatment of Syphilitic Diseases of the Nervous System.*

By J. HUGHLINGS JACKSON, M.D., Assistant Physician to the London Hospital, and to the Hospital for Epilepsy and Paralysis.

(*The Lancet*, October 27, 1866.)

After speaking of the researches of Wilks and Bristowe, Zambaco and Lacharrière on syphilitic disease of the cerebral arteries, Dr. Hughlings Jackson makes the following remarks on treatment of syphilitic nervous affections as reported in the *Mirror of the Lancet*:—

Whilst admitting that nerve tissue might directly suffer from syphilis, he himself knew nothing of syphilitic affections of the nervous system otherwise than as arising from disease of its blood-vessels and of its pia mater. In short, syphilitic diseases of the nervous system were rather diseases of connective tissue *in* or about nervous organs. He remarks on the vast importance for treatment of a knowledge that syphilis might occasionally be the cause of nervous symptoms, and on how much we, as members of a utilitarian as well as of a scientific profession, are indebted to such men as Reade (of Belfast), Todd, Wilks, Bristowe, Hutchinson, and Russell (of Birmingham). He adds, however, that he feared we frequently saw syphilitic affections of the nervous system, and especially epileptiform seizures, too late for very successful treatment. When a man has a unilateral convulsion—and convulsions due to syphilis are generally unilateral—in the midst of ordinary health, it is a sign, Dr. Hughlings Jackson thinks, that there has been disease in the head for some time before it began, as it were, “to live an outward life” in occasional convulsive attacks. Iodide of potassium would be about as likely to absorb “tubercle” as to absorb some of the so-called “deposits” from syphilis. And, as a matter of fact, Dr. Jackson has not found the treatment of *chronic* cases of syphilitic disease of the nervous system to be so satisfactory as many would, *à priori*, think it would be. He believes, too, that iodide of potassium not unfrequently gets more credit than is its due when administered for some of these syphilitic diseases of the nervous system. Patients the subject of what has been called “syphilitic epilepsy” are liable, like other epileptic patients, to have attacks of convulsions in batches, and in the intervals of the seizures they may be almost comatose. Now, Dr. Jackson thinks it is not exact to suppose when a patient recovers from such a condition after taking the iodide of potassium, that he recovers *because* he took it. As a rule, the patient will get over the status epilepticus, whether he takes the iodide or not, and whether the convulsive attacks be dependent on limited organic disease from syphilis or on any other kind of limited organic disease of the hemisphere. The following case is then related:—

James P— was admitted on a Tuesday, and died on the Friday following. The only time when Dr. Hughlings Jackson saw him was in

the afternoon of the day of admission. The patient was apparently quite sensible, and answered questions rationally, but seemed surly and did not like to talk. He was hemiplegic on the left side. No paralysis of the orbicularis palpebrarum could be detected, but there was a little drawing of the mouth to the right side, and the left arm and leg were paralysed. No loss of sensation could be made out. Unfortunately very little history was obtainable from him, and very little could be afterwards obtained from his wife. He had had sore-throat five years before and a scaly rash since, and had been in this hospital twice before for "rheumatism." The man seemed all along to be chiefly dull, and lay quiet; but one of his fellow-patients said he was "curious" in his talk, and instanced that he had said he would like to have "a pound of sausages and two pints of beer all chopped up together." His death took place rather unexpectedly.

No disease was found except in the cranium. The carotid arteries at each side were, from the point at which they were cut to the giving off of the anterior and middle cerebral arteries, much thickened; and this thickening extended about a quarter of an inch up each vessel in a sort of node, ending abruptly. The right middle cerebral artery was much narrowed at one point, and was blocked up, but the coagulum seemed to be recent; the calibre of the left, although apparently the vessel was quite as much diseased outside, was not diminished, and there was no coagulum of any sort in it. Soon after its commencement, the left anterior cerebral artery was swollen into a node the size of a large pea. Both vertebral arteries were thickened and nodose, and the channel of the left one was narrowed at one point, and at this point the artery was fastened to the medulla oblongata by a sort of stuff which looked like dirty putty. The basilar artery was generally thickened, but was not patchy; the posterior communicating arteries appeared to be healthy. As there was a good deal of softening of both posterior lobes, the posterior cerebral arteries were carefully examined, and each soon after its origin was found to have become suddenly nodose, and of about the size of a small horse-bean. The right was firmly blocked near, and the other within half an inch of, its origin. The convolutions in the neighbourhood of the corpora striata were carefully examined. On the left side no softening nor any abnormal state could be detected, and the corpus striatum itself was quite sound. On the right side the corpus striatum was found to be much softened, but was only diffuent in a small part externally, and the convolutions on both sides of the Sylvian fissure were much softened. The softening of the convolutions was not well defined. The softening of the posterior lobes seemed to be about equal. In this instance no microscopical examination was made; but in the other cases Dr. Hughlings Jackson had seen the material matting together the arteries, pia mater, and nerves, was found to consist chiefly of granules and fat, and in some parts a few fibre-cells were found.

ART. 53.—*Aphasia Associated with Right Hemiplegia.*

By EDWARD LONG FOX, M.D., Physician to the Bristol Royal Infirmary.

(*The Lancet*, August 11, 1866.)

The following cases, although very briefly related, are of great interest as bearing on the disputed questions of the localization of faculties. The case of Wm. D. is especially interesting in reference to the ejaculations these speechless patients uttered:—

Mary M——, aged thirteen, was in her usual health at the beginning of May. She was sent by her mother to a shop, and on her way fell down insensible. She was found to be hemiplegic on the right side, and to have lost all articulate speech.

On admission into the Bristol Royal Infirmary, June 5th, 1866, the right hemiplegia was almost complete. She could not lift the right arm at all, nor flex the fingers; and she could scarcely stand. She could not walk at all. Slight facial paralysis of right side. No paralysis of tongue. Organs of special sense unaffected. Pupils natural. Sensation good everywhere. There was no loss of the general faculty of speech, as she could express herself fairly well by signs, and nodded or shook the head rightly. She evidently understood all that was said to her, and the expression of the face was very bright. But she could not pronounce anything except “ta” and “to,” and had lost all power of saying another syllable. She had never suffered from rheumatism; had no valvular disease of the heart; and was a pale, cachectic-looking child.

June 30th.—She had so far recovered as to be able to walk about and to move the arm well, but she could not clasp with the fingers. She had learned several words, as “yes,” “no,” “tea,” “bun,” &c. She seemed very bright, and read to herself with evident pleasure.

Two or three days after this her mother secretly brought her in some cheese and port wine. She became intoxicated, fell into a succession of convulsive attacks, and sank during the night.

We were only able to examine the head:—Much black discoloration along the fissure of Sylvius on the left side. Beneath this region was an old clot, partially discoloured, with creamy brain-tissue all round it, the softening extending slightly into the anterior lobe, but mostly into the middle lobe, and implicating the corpus striatum, except the upper layer of it. This clot was evidently the result of a ruptured left middle cerebral artery. Just at the commencement of the fissure of Sylvius the artery was enlarged to the size of a small haricot bean, and this enlarged portion had given way. No plug was found in any of the vessels. The posterior portion of the third left frontal convolution was discoloured for about one-fifth of an inch in depth, and sensibly softened over a space the size of a small walnut. The immediate cause of death was the rupture of a vessel in the left lateral ventricle, on the upper and outer side of it, just above the corpus striatum. The whole of this ventricle was full of clot, which filled all the cornua, and had found its way to a small extent into the right lateral ventricle. The rest of the brain was healthy. Considering the amount of lesion in

the corpus striatum, it seems extraordinary that she should have recovered the use of the limbs so well.

Wm. D —, aged forty-seven; a great drunkard. Right hemiplegia, with total loss of speech, except that he utters the words, "yes," "no," "pooh," "Lord-a-mercy." These he uses indifferently to express everything; and when he is in a passion he will ejaculate "Lord-a-mercy," evidently with the idea that he is using an oath. He can to some extent express himself by signs; as when he can escape from his wife's surveillance he is able to get spirits from a public-house, and pay for them without any assistance. He can write his own name, and understand all that goes on. He has been quite three years in this state; and although the hemiplegia is much diminished, the loss of speech is the same.

John S —, aged thirty-five. At ten years old he had right hemiplegia, and loss of speech for three months. For six months afterwards he had to learn to speak like a little child. He has now contractions of the right wrist.

Although both these cases are still living, it is probable that they have suffered from a similar lesion to that which caused the symptoms in the first patient, especially when we view the symptoms in connexion with the case quoted by S. Van der Kolk (New Syd. Soc., vol. iv., p. 165, note). "In consequence of a wound, a long splinter from the os frontis above the left eye compressed the anterior part of the left hemisphere, subsequently causing loss of speech; which faculty was completely regained after the removal of the fragment by the trepan."

Some very striking cases, mentioned by Abercrombie and Andral, show at least the connexion between the symptoms of loss of speech and lesion of some portion of the left frontal convolutions.

"Not one," Dr. Fox remarks, "can study Broca's views, nor the very excellent papers which Dr. Hughlings Jackson has written on this subject, without acknowledging that there is considerable evidence in favour of the seat of the faculty of articulate speech being located in the posterior portion of the third left frontal convolution. Dr. Hughlings Jackson's papers are written with so much scientific accuracy and with so much candour and fairness, that they are models of the result of medical observation; and in a late paper he modifies his adhesion to Broca's views. But, as far as the subject has been investigated at present, I think it has only been proved that this portion of the brain is one of the seats of this faculty, and not the only seat."

In confirmation of this view Dr. Fox mentions eleven cases recorded by Andral and others.

ART. 54.—*Aphasia, with Right Hemiplegia. Lesion in the Island of Reil, extending into the External Frontal Convolution.*

By WILLIAM R. SANDERS, M.D., F.R.C.P.E., Physician to the Royal Infirmary of Edinburgh; Lecturer on Physiology and on Clinical Medicine.

(*The Lancet*, June 16, 1866.)

In this communication Dr. Sanders brings forward the case of a woman who became hemiplegic and speechless in August, 1865, and

who died April, 1866. She never uttered a single word from the time of her seizure to her death. Her intelligence was not materially impaired; Dr. Sanders's impression being that her mind retained its essential powers of thinking, feeling, and willing. She could not write. She could make herself understood by her husband; and the ward-nurse said they actually quarrelled.

At the post-mortem examination two softened patches were found in the left hemisphere, and these Dr. Sanders carefully describes.

But the principal lesion was found in the base of the brain, consisting of a softening of the external and anterior portion of the left island of Reil, which extended for a short way into the inferior surface of the adjoining external or inferior frontal convolution, and also penetrated in depth to the outer part of the corpus striatum, which likewise exhibited a small softening at the posterior part of its grey matter. These important lesions were carefully examined, in conjunction with Wm. Turner, M.B., demonstrator of anatomy in the University of Edinburgh, who reported as follows:—

“When the tip of the left temporo-sphenoidal lobe is raised, an alteration in the normal appearance of the island of Reil is seen in its antero-external part. Instead of bulging in a well-defined convex manner, it presents a deep sinking or excavation in the seat of its two most anterior and external gyri; and the surfaces of these gyri have in part lost their normal grey appearance, presenting a distinctly yellow hue. The yellow discoloration extends from the surface of these gyri of the island outwards along the bottom of the groove which separates them from the inferior frontal gyrus, and is continued for about half an inch outwards and three quarters of an inch from behind forwards along the under surface of that convolution. It does not extend to the outer surface. The depressed and discoloured portions of the island, as well as the affected part of the frontal gyrus, feel soft and fluctuating to the touch. There is no erosion nor solution of continuity on the surface of the affected convolution, which remains quite smooth. Owing to the destruction of the convolutions of the island of Reil, already described, a much larger portion of the under surface of the inferior frontal gyrus is exposed, when the temporo-sphenoidal lobe is drawn on one side, than is normal. After the brain had been immersed in spirit for some hours, the characteristic yellow appearance already described was still preserved.

“On a section being carefully made from the island inwards towards the corpus striatum, the softening, which presented an eroded and pul-taceous appearance, was found to have penetrated into the anterior, outer, and inferior part of the corpus striatum. On horizontal section, a second small isolated softening was found at the posterior part of the corpus striatum. In the softened portions, the nerve-structures were destroyed, and replaced by free granules and granular cells.”

Dr. Sanders then remarks:—

“The dissection in this case partly agrees and partly differs from M. Broca's views, which localize the lesion of aphasia in the posterior part of the inferior left frontal convolution. This convolution was over a small space distinctly softened and diseased; but, on the other hand, the chief lesion was undoubtedly seated in the gyri of the island of Reil;

and when it is considered that the speech was not merely impaired, but entirely lost, the question is suggested, whether, supposing that there is a distinct localization of the lesion in aphasia, the island of Reil may not be the spot. The island is a well defined lobule of the brain—it is even named the central lobe by recent authors, from being the centre round which the principal convolutions of the cerebrum are arranged. The whole question of localization, however, is still open.

“In addition to the case now recorded, I have, since the case of asphasia presenting Broca’s lesion, which was published in the *Edinburgh Medical Journal* in March last (p. 811), been obligingly asked by my medical friends to see five cases of aphasia (two under Dr. James Struthers, of Leith, one under Benjamin Bell, Esq., F.R.C.S.E., and two under Dr. M’Gregor, of Tranent); and I have received particulars of other three cases. In all of these eight the aphasia was associated with hemiplegia, and always of the right side of the body. Indeed, the conjunction of aphasia with *right* hemiplegia, and not with left, first pointed out by Dax, rediscovered by Broca, and clearly evinced in the remarkable collection of cases published by Dr. Hughlings Jackson in his excellent paper in the *London Hospital Reports* (vol. i. 1864), in which this subject was first prominently brought before the profession in this country, becomes more completely established the more the disease is studied. Exceptional cases will doubtless occur; it will be of immense consequence that they be accurately observed and described.

Since writing the above, I have found in Dr. Hughlings Jackson’s paper, published in the April number of the *Ophthalmic Review* (at p. 50, top), notice of three cases of aphasia and right hemiplegia in which the lesion extended to and involved the island of Reil. I would also refer to Dr. J. W. Ogle’s collection of cases, especially Nos. 179, 215, 248, and 257, in the *Medico-Chirurgical Review* for 1865.”

ART. 55.—*On Neuralgia of the Fifth Nerve belonging to the period of Bodily Development.*

By FRANCIS E. ANSTIE, M.D. Lond., F.R.C.P., Senior Assistant-Physician to the Westminster Hospital.

(*The Lancet*, July 14, 1866.)

On this subject Dr. Anstie remarks: “The period of bodily development includes, of course, the whole time from birth up to the twenty-fifth year (roughly speaking). But that portion of it which is antecedent to puberty presents few or no cases of facial nerve-pains in which the state of the nerve is the central pathological fact. From the moment when puberty arrives, however, all is changed. In the stir and tumult which pervade the organism, and especially in the enormous diversion of its nutritive and formative *visus* to the development of the generative organs and the sexual instincts, the delicate apparatus of the co-ordinating nervous centres is apt to be overwhelmed (or rather left behind) in the race of

development. The most frequent of the painful affections of the fifth nerve which are traceable to this source is *migraine*, or sick headache. Its clinical history is as follows:—Under the pressure of the bodily influences already referred to, and often of a further debility induced by a precocious straining of the mental powers, the patient begins to suffer headaches after any unusual fatigue or excitement, sometimes without any distinct and obvious cause of this kind. The unilateral character of the pain is not always detected at first; but as the attacks increase in severity it becomes obvious that the pain is limited to the track of the supraorbital, and sometimes the ocular, branches of the ophthalmic division of the fifth nerve of one side. In very rare cases, however, as with any other form of trifacial neuralgia, the nerves of both sides are affected. If the pain lasts for any considerable length of time, nausea, and at length vomiting, are induced. This is followed at the moment by an increase in the severity of pain; but from this point the violence of the affection usually so far relaxes that the patient soon succeeds in falling asleep. [The history of the attacks distinctly negatives the idea that the vomiting is ordinarily remedial. This symptom merely indicates the point of lowest depression of nervous power; but as the power of digestion is almost entirely suspended during the attack, it may sometimes happen that a quantity of food which has been incautiously taken, lying as it does undigested in the stomach, may of itself greatly aggravate the headache by irritation transmitted to the medulla oblongata. In such a case vomiting may produce direct relief to the nerve-pain.] When the patient awakes from sleep, the active pain is gone. But it is a common occurrence, indeed it always happens when the pain has lasted a considerable time, that a *tender* condition of the superficial parts remains for some hours, or even a day or two. This tenderness is diffused over a considerable surface, and is nowhere so exquisite as that which is observed in the “painful points” of Valleix, which are developed in the severer neuralgia. Sick-headache is not uncommonly ushered in by sighing, yawning, and *shuddering*—symptoms which remind us of the prodromata of some graver nervous affections, with which I shall hereafter indicate its probable relationship.

“Another kind of headache which infests the period of bodily development is that which is known as the *clavus hystericus* (*clavus* from the fact that the pain is at once very severe, and is limited to one or two small definite points, as though a *nail* or nails had been driven into the skull). As Valleix has well shown, the points to which the pain is confined in these cases correspond with one or more of the localities which are the *foci* of severest pain and tenderness in all forms of trifacial nerve-pain. But for the greater limitation of the area of the nerve-pain, there would be scarcely any important distinction between *clavus* and *migraine*; for the former, when the attack is unusually severe and prolonged, generally culminates in a fit of vomiting, just like that of ordinary sick headache, and is followed by a superficial tenderness, only more limited in extent than the soreness which follows sick headache. The adjective *hystericus* is of course an inadequate and improper definition of the circumstances under which this form of trifacial nerve-pain arises. The truth is that the subjects of *clavus* are usually

females who are passing through the trying period of life between puberty and the complete development of the organism ; but there is no evidence to show that disorders of the uterine functions give any special bias towards this complaint. Both migraine and clavus are met with frequently enough in persons who have long passed the period of bodily development. But the important circumstance to observe is, that the tendency to these forms of nerve-pain nearly always shows itself during that period.

“It seems superfluous to insert a caution to the effect that true migraine has no connexion with ordinary “bilious” headache, which is the mere result of disordered digestion. Yet the carelessness with which the term “sick headache” is applied now to the one and now to the other affection, by some writers, leads me to notice the danger of this mistake.”

(B) CONCERNING THE RESPIRATORY SYSTEM.

ART. 56.—*On Bloodletting in the Treatment of Pneumonia.*

By Dr. ALEXANDER SMITH, Surgeon Royal Artillery.

(*Edinburgh Medical Journal*, July, 1865.)

In a valuable paper recording the results of observation of one hundred and eight cases of pneumonia, treated in the hospital of the 47th Regiment, at various stations in Canada, and of whom three died, Dr. Smith remarks that his experience of the effects of bloodletting convinced him that its employment at the outset of pneumonia in its sthenic form was attended with most beneficial results, not only in shortening the duration of the disease, and rendering convalescence satisfactory, but also in giving an amount of certainty and uniformity to the results of treatment which could not be attained by the employment of any other combination of remedies. As to its power in “cutting short” the disease—if by this term is meant to be expressed the probability of its at once arresting, and as it were stamping it out—Dr. Smith’s experience would go to show that its employment is not attended with any such result. In proof of this, he mentions that so soon as he became aware of the import of the condition of the respiration, which is first observed at the outset of pneumonia, he attempted, by early bleeding, before the disease had advanced beyond the stage indicated by obscurity of the respiratory sounds, to arrest it in that of engorgement. In no case, however, was this practice attended with the result desired ; but, on the contrary, in every attack so treated, instead of being altogether prevented, small crepitation seemed to undergo an earlier development. The subsequent progress of all such cases early bled was otherwise invariably satisfactory.

Dr. Smith, however, still feels inclined to consider this question in the light of an open one, and to believe, until distinct proof to the contrary shall have been produced, that bloodletting practised soon after the occurrence of the rigor may possibly at once arrest the disease. He

is the more inclined to this view of the matter, because Dr. Jameson, his colleague in the 47th Regiment, informed him, that in one case which he bled freely immediately on the man's admission into hospital, and within a very short time of the occurrence of an attack of rigor, which from all the attending circumstances, and happening as it did at a time when pneumonia was prevalent among the men of the corps, appeared to be the initial symptom of an attack of that disease, no further indisposition followed. This may or may not have been a case which, if it had not been so treated, would have proved one of pneumonia; but still Dr. Smith believes the fact is worth recording.

Dr. Smith thinks that it was by limiting the stage to which the diseased action advanced, rather than by affecting the extent of lung to be attacked, that bloodletting manifested its power to shorten the duration of the disease. That it also influenced the amount of lung attacked, however, he thinks, appears evident, from what was found to have happened in some of the fatal cases, neither of which were bled at the outset of the disease. With regard to the extent of lung affected in cases early bled, it may be said, that it amounted, as a general rule, to from one-half to three-fourths; and that in respect of the part first attacked, in no instance did the disease begin at the apex.

After having most carefully watched the whole course of the disease in attacks where bloodletting was employed at the outset, Dr. Smith feels satisfied that in no case so treated did red hepatization take place; both the exaggerated respiratory sound heard near the acme of engorgement, as well as the absence of evidence of the entrance of air, excepting during forced respiration, which frequently for a few hours preceded the setting in of small crepitation, having been unconnected with any degree of actual consolidation. Neither were the bronchitic sonorous râles occasionally audible along with large and small crepitation near the middle of the lung, in the course of some of the cases, confounded with the blowing sound of bronchial respiration heard when true hepatization was present. The facts of greatest importance, however, noticed with reference to the employment of bloodletting, were the rapidity with which such cases recovered in proportion to the severity of the attacks, and the uniformity of the results observed on a review of the whole cases so treated, as compared with that obtained in the milder and more asthenic attacks in which bloodletting was not made use of. This is shown by tabular statements given in the paper.

A further consideration, possessing also considerable practical importance, is the fact, that in cases not bled it was found, Dr. Smith states, that there existed, throughout the greater part of the attack, a danger that a fresh accession of fever, and a rapid advance to hepatization, might not only suddenly occur, but do so at a period of the disease when good results from bloodletting, if it should then be employed, were but little likely to be obtained. Dr. Smith, however, wishes it to be distinctly understood, that whilst advocating the employment of bloodletting at the outset of sthenic cases of pneumonia, such as are seen in young and previously healthy soldiers, and whilst maintaining also from actual observation that the good results which follow such a mode of treatment surpass in a marked degree those obtained from any other combination of remedies, he does not in any way call in question the

value of that mode of treatment termed "restorative," as applied to a particular class of cases, and which has been employed with so much success in the management of the pneumonia seen in civil hospitals in Britain.

ART. 57.—*On Anthracosis, or Coal-miners' Phthisis.*

By Dr. J. WARBURTON BEGBIE, F.R.C.P. Edin.

(*The Glasgow Medical Journal*, September, 1866.)

In an examination of this subject, Dr. Begbie has endeavoured to signalize the chief features, whether of pathological or etiological interest. As a result of his examination he is disposed to conclude—

1. That anthracosis is primarily determined by the inhalation of carbonaceous particles.

2. That in the instance of the coal-miner, while capable of being produced in various ways, the chief exciting cause is the inhalation of the very impure atmosphere occasioned by the burning of oil lamps. It would appear that the long-continued inhalation of a very *dusty* atmosphere may, under certain circumstances, engender the same condition.

3. That when once the deposition of carbon in the pulmonary structure has taken place to any extent, and the true function of respiration is thereby interfered with, there occurs a tendency which gradually increases to the arrestment of carbon or carbonaceous pigment in the lungs, and its removal there from the blood.

4. That the presence of black pigmentary deposits in the bronchial glands, the pleura, and less frequently the peritoneum and mesenteric glands, makes it probable that there may, in cases of anthracosis, be some peculiar process of carbonaceous absorption as well as deposition of carbon.

5. That in this view, the opinion as to the black pulmonary deposit being the result of transformation in hæmatin, although supported by so distinguished an observer as Virchow, cannot be considered as so readily reconcilable with what we know of the natural history, and especially the etiology, of the disease.

ART. 58.—*On Myeloid Transformation of the Lung.*

By T. C. ALLBUTT, M.B., Physician to the Leeds Fever Hospital.

(*The Medical Press and Circular*, July 11, 1866.)

The following, perhaps unique, case of complete myeloid transformation of the lung occurred under the author's care in the Leeds Infirmary:—

During life there were found complete dulness and stillness all over

the left chest, and absence of vocal sounds and fremitus; or at least these, from the feebleness of the subject and the distance of the voice, were indefinable. The heart was seen to beat under the right nipple. Cough was almost absent, and there was no great dyspnoea. The intercostal spaces were not bulged, and the circumference of the left chest only exceeded that of the right by three-quarters of an inch. There was some degree of emaciation and of hectic fever present. The duration of the disease was uncertain, but certainly of eighteen months' standing. There was no marked cancerous cachexia of appearance, and the progress seemed to have been slow. The boy was fourteen years of age. He remained in the house about eight weeks in the autumn of 1865, and, becoming more and more exhausted and short of breath, he left the hospital for home, where he died in a few weeks. Mr. Jessop performed the post-mortem examination for the author. The whole of the left chest was found filled with solid substance, thrusting the heart out of sight on the right side, and pushing down the diaphragm to the left kidney. The solid contents were of two kinds. The upper portion which appeared on opening the chest was of a dense fibrous character, of a greenish-white colour, and presented the form of an enlarged lung. Below this, occupying the whole back of the chest, and in contact with the costal pleura, was a considerable quantity of true myeloid matter, soft and sanguineous. Both substances contained myriads of little bones, varying from the size of a pea to that of the thumb. These being densely packed in the upper and firmer mass, made it almost impenetrable. The origin of the disease was probably in the chest-walls, and had thence impregnated the lung. No attachment could be found, however, nor disease of ribs or spine. The friends of the lad stated that he had been short-winded since his earliest age, and had presented some prominence of the chest for many years.

ART. 59.—*On the Influence of Age in determining the Liability to Asthma.*

By HYDE SALTER, M.D., F.R.S., Physician to Charing Cross Hospital.

(*The Lancet*, July 28, 1866.)

The age of the patient is not, Dr. Hyde Salter states, important, and has very little direct instructiveness. His cases merely show that asthma comes under the physician's care at every period of life, and that the largest number of cases are in what may be called middle life—from twenty to fifty. There are more asthmatics of ages between thirty and forty than any other equal period. On each side of these ages—before thirty and after forty—the numbers fall.

A much more interesting and important point is the time of life at which asthma makes its appearance. On this point Dr. Hyde Salter's cases show the following facts:—That, dividing life into equal intervals of ten years, a larger number of cases take their commencement in the

first ten years of life than in any subsequent equal period; that childhood is of all ages the most prolific of asthma. After childhood there is a sudden fall; during adolescence much fewer cases declare themselves. But from this there is a gradual rise up to forty. Thus, the number of cases in which the disease commenced between ten and twenty was 20; between twenty and thirty, 23; and between thirty and forty, 27. From forty to seventy, again, there is a regular but rapid fall. Thus the number of cases commencing between forty and fifty was 16; between fifty and sixty, 9; and between sixty and seventy, 3. We should naturally expect that the time of life at which asthma was apt to show itself would be closely connected with the causation of the disease; and so Dr. Hyde Salter thinks it is. For he arrives, from the foregoing figures, at the following conclusions:—

“1st. That the time of life the most prolific of asthma is the time of measles, of whooping-cough, and of infantile bronchitis.

“2nd. That adolescence furnishes comparatively few cases, because the diseases of childhood, so apt to lay the foundation of it, are over, while the wear and tear and hardships of life, and the deterioration of the body produced by them and by time, have not commenced.

“3rd. That from this time exposure and hardship and time begin to tell, and show their influence by the increasing asthma-rate reaching its maximum at middle life. But, it may be asked, why should the tendency for asthma to show itself increase up to forty, and then diminish? Why should it not go on increasing as life advances, especially as we know that the tendency of catarrhal and other agencies to produce inflammatory conditions of the respiratory mucous membrane does increase up to the very end of life?

“This brings me,” Dr. Hyde Salter continues, “to the fourth point, which is this: That this diminishing probability of asthma making its first appearance after middle life shows that it does not follow the same law as bronchitis, and that there is something necessary for its development besides vascular change in the bronchial tubes and other organic lung mischief. This other thing is doubtless the asthmatic tendency or idiosyncrasy; and the way in which the necessity of the asthmatic idiosyncrasy for the production of the disease accounts for the diminishing probability, as life advances, of its making its first appearance is this: As every year is added, an individual is decreasingly likely to be exposed *for the first time* to the exciting cause of the disease; if any one has the predisposing cause—the asthmatic tendency—within him, it is not likely he will travel far through life without the exciting cause presenting itself and bringing the disease into activity, and those only can reach advanced life without becoming asthmatic in whom either the asthmatic tendency is *nil* or feeble, or who have fortuitously escaped circumstances calculated to call it into activity. Such a number must, according to the doctrine of chances, be a constantly decreasing series. The diminishing number of cases is, in fact, an exact measure of the diminishing probability of a person with the asthmatic tendency postponing his first exposure to exciting causes to so late a date.

“There is nothing in relation to asthma about which more misconception prevails than the time of life at which it is apt to occur. It is commonly thought to be a disease of old age, and we frequently hear

the expression, 'as asthmatic as an old man.' I believe there are two reasons for this error. One, that asthma, if it is not lost comparatively early in life, or if it comes on in middle life, is generally never lost, and therefore exists in old age; and thus many old people are truly asthmatic simply because they have never ceased to be so. The other reason is that chronic bronchitis—undoubtedly a disease of advanced life—is often mistaken for asthma; an old man coughs and wheezes and spits, and is said to have the asthma; but he has really chronic bronchitis; and although the bronchitis may have a little bronchial spasm superadded to it, or even a great deal, still it is essentially and substantively bronchitis.

"The error of imagining that asthma is not a disease of early life is one into which, as I have shown, even medical authorities have fallen. But my cases furnish abundant evidence that asthma may, and frequently does, occur very early in life; that there is no time of life that is free from it; and that it may occur even in infancy."

ART. 60.—*The Teleology of Pulmonary Consumption.*

By Dr. J. HENRY BENNET.

(*The Lancet*, September 22, 1866.)

Teleological views of disease are sufficiently rare at the present day to deserve especial note when met with. Dr. Henry Bennet, although anxiously desirous to promulgate the belief that "pulmonary consumption is a curable disease—indeed, in its early stages a very curable disease—under proper treatment," does not seek to hide the intrinsic gravity of the malady, but sets this forth in the following manner, appending also certain teleological considerations:—

"In the investigation of the nature and causes of pulmonary consumption, and of tubercular disease in general, we may perhaps go a step further. I firmly believe that the appearance of tubercular deposit ought to be looked upon as the evidence and result of a serious, perhaps final, diminution of vital or nervous energy. In other words, it may be considered the evidence of incipient decay of the organization from defective vital or nervous power. Thus tuberculization, especially when seated in the lungs, is simply a mode of dying. Unless the vitality of the individual can be roused, the morbid condition will surely progress, and life will be extinguished sooner or later, according to the state of the constitution of the patient, and of the consequent type of the disease.

"The very essence of life is the organic vitality, variable in different species, variable in different individuals, with which each organism, vegetable or animal, merges into being and develops itself. It is owing to inherent organic vitality that the medium duration of life in the oak, the ash, the fir is different, as it is also different in the whale, the elephant, the horse, the dog, and in man himself. The medium duration of life in each species is reached in the organisms that are created

under favourable conditions, with unimpaired organic vitality, and that pursue their existence under conditions favourable to life. On the other hand, this medium duration is not reached by those individuals that are created under unfavourable conditions, with defective vitality, or in whom originally sound vitality is modified, diminished, destroyed by the unfavourable conditions in which their existence is carried on.

“In such considerations, in my opinion, must we seek for the real explanation of tubercular disease, and especially of pulmonary tuberculization; as also for a key to the types under which the disease presents itself, and to the results of treatment. They include, of course, hereditary predisposition.

“Viewed in this light, so far from pulmonary consumption being a dire inexplicable pestilence, striking indiscriminately the young and the old, it becomes one of the provisions by which Providence has secured the integrity of the human race. If those who are, from birth or otherwise, sickly or weak, in whom vitality is defective originally or secondarily and accidentally, could propagate their kind so that their progeny could live, the human race would soon degenerate and become a race of pigmies, of sickly dwarfs, and eventually die out. Pulmonary tuberculization is in reality one of the diseases by which Providence eliminates those that are weak, imperfect, and consequently unfit to perpetuate the race in its integrity. Individually it may be very hard to be thus eliminated for the good of the human race; but if we rise above individuals and grasp the interests and well-being of the entire human family, it will be seen that these diseases are, in truth, a bountiful dispensation of Providence. They may be compared to hurricanes in tropical climates, which purify the earth and contribute to make it habitable, although often at the expense of great individual suffering.”

ART. 61.—*On the Use of the Thermometer in Tubercular Phthisis.*

By Dr. ———

(*The Medical Times and Gazette*, June 23, 1866.)

Few diseases can cause a daily elevation of three or four weeks' duration. Acute inflammations cease, and the temperature consequently falls, long before this period has elapsed. The same remark applies to most of the acute specific fevers. The temperature in typhoid fever generally becomes normal by the twenty-fifth or thirtieth day of the disease. The diseases at present known to be able to cause such a long-continued elevation of the temperature as that above mentioned (namely, a month or more) are tuberculosis (the deposition of tubercle in any of the organs of the body), rheumatism, ague, abscesses, suppuration (such as occurs in empyema, large open psoas abscesses, &c.), and certain forms of chronic induration of the lung, with ulceration of the bronchi and the formation of cavities.

Under the term tuberculosis the writer includes scrofulous pneumonia,

the product of which disease was formerly, and still is by some, considered to be one of the forms of yellow tubercle.

All these diseases, with the exception of tuberculosis, are accompanied by such characteristic symptoms that there is usually no difficulty in forming a correct diagnosis. Rheumatism makes itself known by the pain in the joint, or by the physical sign of peri- or endo-carditis. The symptoms of ague are mostly so characteristic that the disease can seldom be mistaken: abscesses, empyema, profuse suppuration, can always be detected (abscesses may evade detection, however, for some time). Of all the diseases mentioned, chronic induration of the lungs, both in respect of physical signs and symptoms, closely simulates phthisis.

The length of time, therefore, that the elevation of the temperature continues affords much help in making our diagnosis. If this elevation has continued some time—say a month—the number of diseases we have to decide between is small, and these for the most part have characteristic symptoms, and thus the diagnosis becomes easy. Tuberculosis, however, may exist, without any physical signs being present, and at the same time the symptoms may be very slight and utterly insufficient for a correct diagnosis. In such a case, if the patient continue febrile for a month or six weeks, the disease is in all probability tubercular.

In cases where there is a doubt whether the patient suffers from phthisis or not, if the temperature be elevated, tubercle is probably being deposited in one or more of the organs of the body. Such elevation may be due to some co-existing febrile disease not tubercular; but such a disease mostly ceases in a few days, and in the case of typhoid fever usually at the end of the fourth week. If, therefore, the temperature continues to rise daily for more than a month, we are justified in diagnosing in such a case (provided none of the other diseases capable of producing a chronic elevation of the temperature be present) that the patient is tubercular.

It may be said that before a month has elapsed the physical signs and symptoms will be so marked that all difficulty of diagnosis will have ceased. Such, however, is not the case, for tuberculosis of the lungs or other organs of the body may continue for a much longer period than that mentioned, and yet produce no physical signs, while the symptoms, moreover, may be slight and utterly insufficient to insure a correct diagnosis.

Hence it follows that in some cases the temperature of the body affords us the earliest indication that tubercle is being deposited in the body; *for if there is a chronic elevation of the temperature, and this be not due to rheumatism, ague, suppuration, or chronic induration of the lung, such an elevation must be considered to be due to a tubercular deposit in the body.*

ART. 62.—*On the Detection of Lung-Tissue in the Expectoration of Persons affected with Phthisis.*

By SAMUEL FENWICK, M.D.

(*British Medical Journal*, July, 1866.)

In a paper read before the Medico-Chirurgical Society the author stated that he had included in his observations the results obtained from the examination by the microscope of the expectoration of one hundred real or suspected cases of phthisis. The plan hitherto recommended of searching for pulmonary tissue in sputum had been to spread it on a flat surface, and to pick out of it with needles any portions that might appear likely to contain elastic fibre. He had, on the contrary, been in the habit of liquefying the expectoration by boiling it with a solution of pure soda, and then placing the fluid in a conical-shaped glass, when every particle of elastic tissue fell to the bottom, and could be removed and placed under the microscope, as in the examination of urinary deposits. In this way he had easily found one-hundredth part of a grain of pulmonary structure after it had been mixed in bronchial mucus; and he calculated that one-thousandth to one six-thousandth part of a grain might be detected in any expectoration that may contain it.

In thirteen out of twenty-three cases in which tubercle was suspected to be in the first stage, lung-tissue was found in the sputum. In seven of the twenty-three cases there was no physical sign of tubercle, but its existence in the lung was suspected from general symptoms only; and in the expectoration from these there was no pulmonary tissue. In sixteen cases there were stethoscopic signs leading to the belief that tubercle was present; and in thirteen of them elastic fibre was found in the mucus coughed up.

There were twenty-four cases in which auscultation and percussion indicated softening of tubercle in the lungs, and in all pulmonary tissue was present in the sputa. In fifteen the physical signs were of a doubtful nature, and seven of these presented microscopic evidence of ulceration of the lungs.

In thirty-five cases the stethoscope indicated cavities, and in all these there were fragments of lung-tissue in the expectoration. In two cases the author had diagnosed enlarged bronchial tubes, and in neither of them was there any appearance of elastic fibre in the sputum. In sixty-nine cases he counted the numbers and size of the fragments of lung expelled. In one specimen, coughed up in twelve hours, eight hundred fragments were found; and often fifty or sixty fragments were detected where, from the stethoscopic signs alone, no great destruction of lung could have been anticipated.

The proportion of bronchial tubes the author found to be least in the stage of softening, and greatest where the stethoscope indicated cavities. The greatest proportion of fragments of single air-cells was

found in the first stage, and the largest proportion of large fragments of lung where cavities existed.

(C) CONCERNING THE CIRCULATORY SYSTEM.

ART. 63.—*On the Application of the Graphical Method to the Study of Diseases of the Heart.*

By Dr. BALTHAZAR FOSTER.

(*Medical Times and Gazette*, September 29, 1866.)

In a first article on the application of the graphical method to the study of the diseases of the heart and great vessels, Dr. Balthazar Foster reproduces M. Marey's figure of the normal tracing of the heart's action obtained by the cardiograph, and explains it as follows:—



The point indicated by the letter A corresponds to the contraction of the auricle. The elevation at B corresponds to the systole of the ventricle, which lasts up to the point C; the intermediate undulations are referred by Marey to the closure of the mitral valves. The point marked by the letter C corresponds to the closure of the sigmoid valves, and the sudden fall in the trace which occurs after this point corresponds to the diastole of the ventricle. The slight undulation D at the beginning of the line of ascent of the trace Marey considers to be due to the sudden influx of blood from the auricle to the ventricle, immediately after the relaxation of the latter. It is no doubt caused by the commencement of the flow into the ventricle, but whether we are to attribute this flow simply to the effect of the relaxation of the ventricle, or to the first part of the contraction of the auricle, is open to discussion.

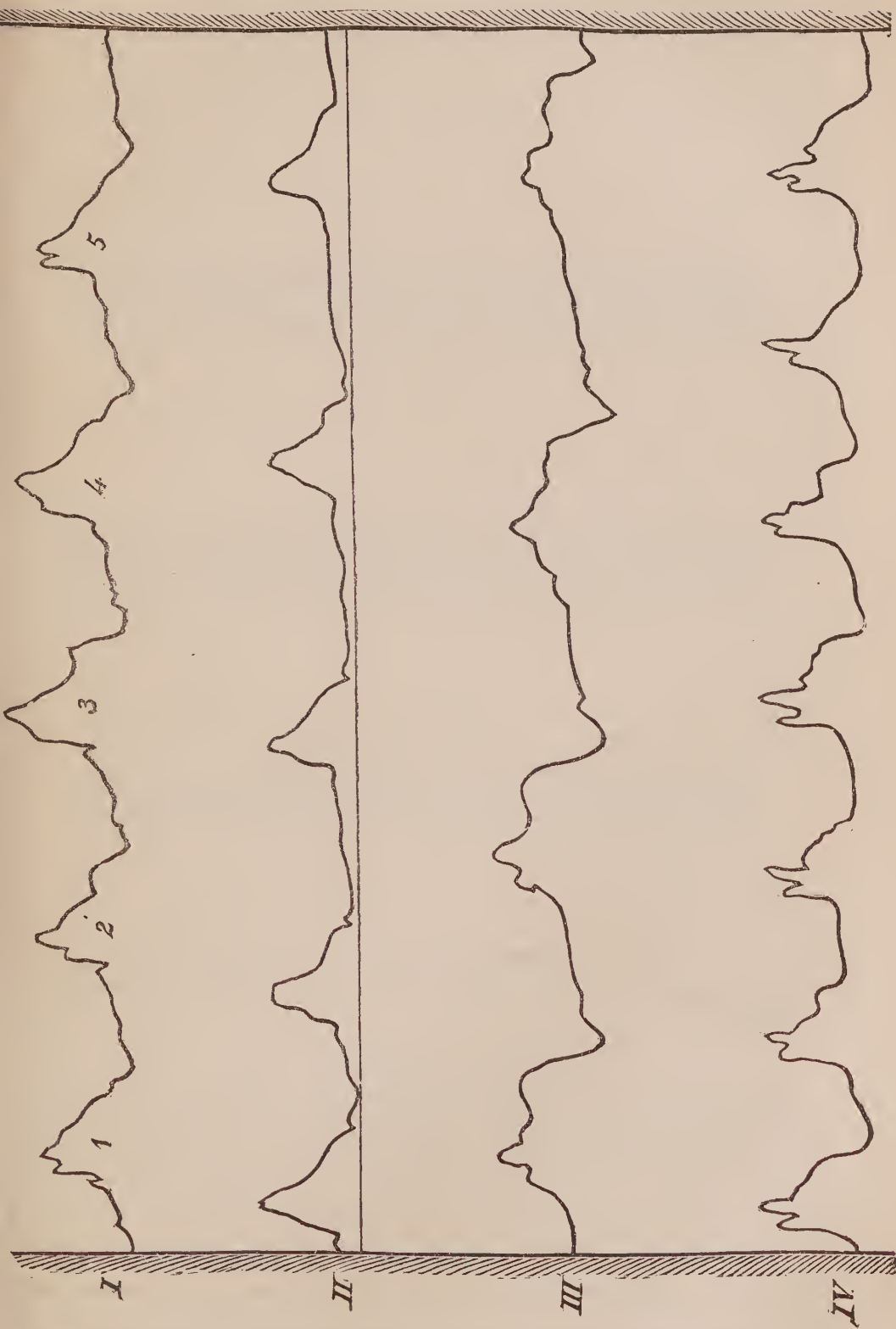
ART. 64.—*On the Cardiograph.*

By M. MAREY.

(*Journal de l'Anat. et de la Phys.* II., 1865; *Schmidt's Jahrbücher*, 1866.)

M. Marey describes some experiments with the cardiograph. He finds—

1. That the trace coincident with inspiration is of greater amplitude—



i.e., of greater distance from its lowest to its highest point, than the trace coincident with expiration.

2. When the breath is held at the end of a deep inspiration, and a trace taken at the same moment, it is seen (A) that the abscisses corresponding to the successive phases of the heart's action are increased in length; that is, that the frequency of the pulsations is diminished; and (B) that the successive curves are of smaller amplitude; that is, that the force of the impulse is diminished. The diminution of amplitude cannot be ascribed to a change in the position of the heart occasioned by the diaphragm, since while the breath is held the heart's place remains the same. The greater fulness of the right chambers, in consequence of the impeded respiration, is the probable cause of the phenomenon.

3. When a strong expiratory effort is made with closed glottis, an effect is produced upon the pulse trace taken immediately after the relaxation of the effort. The cardiographic trace also taken under such conditions differs in many respects from the normal one. Marey ascribes the change to the energetic action of the left ventricle, and to the more rapid rush of the blood.

4. The cardiographic trace taken after strenuous muscular exertion, displays certain peculiarities, essentially due to the diminished energy of the auricular systole, and to rapid closure of the atrio-ventricular valves.

The annexed tracings (p. 105) were obtained under the conditions above specified.

No. 1 is a trace taken during quiet respiration. The curves numbered 1, 2, and 5, correspond to expiration; 3 and 4, with greater amplitude to inspiration.

No. 2 was taken whilst the breath was held after a deep inspiration.

No. 3 was taken after a powerful expiratory effort with closed glottis.

No. 4 was taken after strenuous muscular exertion.

ART. 65.—*On the Theory of the Pulse.*

By Dr. J. BURDON SANDERSON, Senior Assistant-Physician to the Hospital for Consumption, Brompton, and the Middlesex Hospital.

(*The Lancet*, November 10, 1866.)

This paper forms the first of a series on the application of physical methods to the exploration of the movements of the heart and pulse in disease by Dr. Sanderson and Dr. Anstie. The object of the series is to show in what way the sphygmograph may be made useful at the bedside or in the consulting-room. That portion of the present paper which especially relates to the physician does not permit of curtailment.

"5. To the physician, the value of the pulse as a sign depends mainly, though not entirely, on its affording indications of the mode of contraction of the heart. Physiologically, it can only be understood as a *propagated* phenomenon; for whether it is regarded as a tide (*flumen*),

or as a wave (*unda*), it is alike transmitted from the heart towards the periphery. For this reason the investigation of its nature must commence at the aortic orifice, or as near to that point as possible. It originates in the aorta, and is to be found in its entirety. The further from the centre it is explored, the more will it be modified by transmission, and the more difficult will it be to analyse its characters.

FIG. 1.

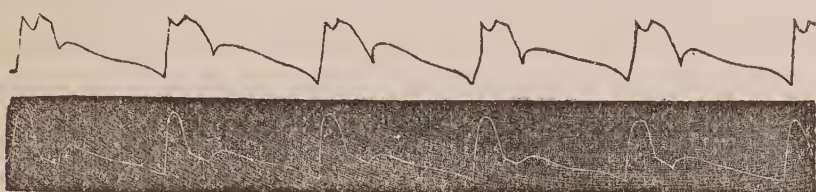


FIG. 2.

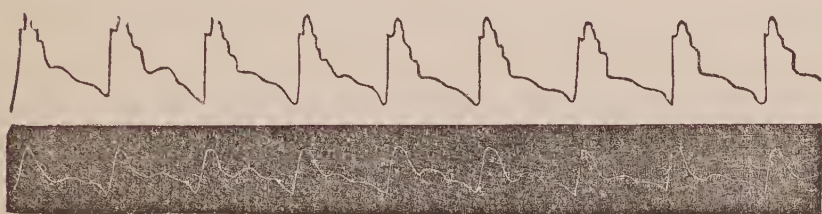


FIG. 3.



"FIG. 1.—Tracings obtained by the successive application of the sphygmograph to the carotid and radial arteries of a male patient, aged twenty-one, under my care at the Middlesex Hospital, convalescent from lead colic; the carotid tracing being on a white ground, the radial on a black.

"FIG. 2.—Carotid and radial tracing of a patient, aged twenty-three, under my care at Brompton, suffering from mitral and aortic disease of the heart.

"FIG. 3.—Similar tracings of a male patient, aged twenty-five, also under my care at Brompton, affected with phthisis in the third stage. The tracings show some of the more striking differences between the pulse-curves of the two arteries, particularly the greater duration of the systolic distension in the carotid than the radial—the peculiarities due to the propagation of vibrations from the heart and the notch or angle by which the moment of closure of the aortic valve is indicated. In each instance a low arterial pressure is indicated.

"6. In man the pulse of the aorta cannot be explored; but the great arteries leading from it are so placed that in most persons the sphygmograph can be so applied as to obtain satisfactory tracings, the forms of which, as might be expected, are more complicated and various than

those yielded by the radial. Considering the size of the carotid, and its proximity to the aortic aperture, it may be assumed that the pulses of these vessels are identical both in time and character to that of the aorta; and that whatever be the theory we adopt as to the pulses of the more remote arteries, we have here to deal with the primary extension of the arterial wall, due to the actual progression of blood from the heart into the aorta.

“7. The curve obtained when the sphygmograph is applied either to the carotid or subclavian in the neck is markedly divided into two parts. One of these has been found by observation to coincide with the period during which the heart is in communication with the aorta; the other, with the interval during which the sigmoid valves remain closed. The commencement of the systolic period is always marked by a sudden jerk upwards of the lever, usually indicated by a nearly vertical line; the close, by a second but more inconsiderable jerk, which is always preceded by a sudden depression. During the first part of the intervening time the artery is distended with blood, but towards the end the distension suddenly ceases. The contraction of the ventricle having terminated, the blood flows back from the aorta into the heart. This back flow lasts only for a moment, being arrested by the closure of the aortic valves. Immediately the communication with the heart is stopped, the aortic pressure again rises, tilting up the lever as already described. Although the carotid pulse-curve exhibits great variety in different individuals, its form always indicates that at each stroke of the heart the lever rises abruptly, remains elevated for a short period, and then falls as suddenly as it rose. That the period of elevation coincides with the actual influx of blood from the ventricle is proved by experiments made by Chauveau, who found that when his beautiful instrument for measuring the velocity of the blood-stream was introduced into the carotid of a horse, to which a sphygmograph was at the same time applied, the acceleration of the current corresponded in degree and duration with the distension of the artery.

“8. Of the less constant characters of the carotid tracing, some are directly dependent on the *mode* in which the ventricle contracts, others on peculiarities in the closure of the valves. Under certain conditions, the nature of which cannot be here discussed, the mass of blood in the ventricle is thrown into rapid vibration during the act of contraction. Whenever this is the case the vibration is communicated to the arteries, and shows itself in the carotid tracing; the ascending limb of the pulse-curve being interrupted by a series of notches, each of which corresponds to a *vibration*. When these vibrations are very well marked, they communicate the sensation of thrill to the finger. The other variations to which reference has been made have to do with the descent of the lever at the end of the systolic period. In some cases this descent is well marked, and is denoted by a deep notch, as in Fig. 3; in others it is marked by a mere change of direction. These differences may be clearly shown to be dependent on the degree to which aortic regurgitation takes place, or, in other words, on the rapidity with which the sigmoid valves close. When the mean arterial pressure is high, the valves close instantaneously; in the contrary condition, when the arteries are flaccid, *the valves remain open for an appreciable period after the ventricular contraction has ceased.*

"9. The preceding observations tend to show that in the great vessels pulsation is produced by a simultaneous *increase of tension* and *acceleration of movement*. In the more distant arteries these two effects are no longer coincident. Both are transmitted; but inasmuch as their propagation is influenced in a directly opposite manner by those physical conditions which exist in the arteries, they tend to separate themselves more and more from each other the further the exploration is made from the heart. Just as a ray of light becomes two by being transmitted through a medium which refracts them at different angles, so the pulse, in its propagation from the heart to the periphery, splits into its two elements of expansion and acceleration.

"10. The limits of this paper do not permit me to show the light which these considerations throw on the explanation of the peculiarity and characters of the radial pulse. In anticipation of my next paper, I place before the reader the following summary of the conclusions I have arrived at with respect to it.

"*a.* The propagation of the systolic acceleration, if it occurs at all, gives rise to a rapid expansion of the artery, which is synchronical with the ventricular systole, and is followed immediately by collapse.

"*b.* The propagation of the systolic pressure-wave gives rise to a *gradual* expansion of the artery, and, in accordance with Weber's law, follows the systole after a variable interval.

"*c.* When the resistance due to the mean arterial tension is considerable, the acceleration extends only to the great vessels, so that its effects at the wrist are imperceptible. In such a case the form of the pulse is wave-like. In the opposite condition as regards arterial tension, the effects due to the systolic acceleration are well marked; the elevation of the lever is abrupt, but of short duration."

ART. 66.—*On the Seat and Mechanism of the Cardiac Murmurs called Anæmic.*

By Dr. PARROT.

(*Archives Générales de Médecine*, Août, 1866.)

Dr. Parrot lays down the following propositions:—

The cardiac murmur called anæmic is very frequent. It not only exists in anæmia, chlorosis, certain cachectic states, hypochondriasis, and hysteria, but also in the majority of cases of acute articular rheumatism.

In fevers it is common to find a cardiac souffle having characters analogous to those of the anæmic murmur.

All these abnormal bruits ordinarily accompany pulsation of the external jugulars.

Contrary to the opinion that they are localized at the commencement of the aorta, Dr. Parrot maintains that their seat is the right auriculo-ventricular orifice, and that they arise from an insufficiency of the tricuspid valve.

To indicate this double fact, Dr. Parrot proposes to call the murmurs in question, forming a well-determined group, *tricuspid*.

The morbid process which gives rise to them is explained by him as follows: (1.) enlargement of the right side of the heart and its vessels from debility and general atony in the different anæmic states indicated, by the immediate intervention of the nervous system in fevers; (2.) consecutive augmentation of the auriculo-ventricular orifice, and consequent insufficiency of the valve.

From this twofold connexion of phenomena results the distinction of tricuspid murmurs into the anæmic or *passive*, and the febrile or *active*.

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 67.—*On Peritoneal Friction-Sounds.*

By Dr. SEIDEL.

(*Schmidt's Jahrbücher*, 1866, No. 4; *Archives Générales de Médecine*, Juin, 1866.)

Peritoneal friction-sounds, Dr. Seidel states as the result of his observations, have been noted on a level with nearly all the abdominal organs. Their signification is very variable. They indicate the existence either of a chronic or an acute malady, but more rarely the latter. It is not necessary for the production of a friction-sound that the peritoneum should be supported, as it were, by a solid part. In the majority of the cases the friction-sound was somewhat rhythmical, under the influence of respiratory movements, which were perceptible even in the hypogastrium. The perihepatic friction-sound, particularly when occurring over the convex surface of the organ, might easily be confounded with a pleuritic friction-sound. To distinguish the one from the other, let the patient make a forced inspiratory movement, the glottis being closed, a movement similar to those which accompany vomiting. In this movement the inferior border of the lung is not sensibly displaced whilst the liver is notably elevated. If the friction-sound remains under these circumstances, it is almost certain that it is of peritoneal, not pleural, origin. In no case has a non-rhythmical, sound arising from the peristaltic movement of the intestines, been noted. Peritoneal friction-sounds are observed of every grade, from an extremely slight rustling to a veritable rasp.

ART. 68.—*Milk-Diet and Onion-Juice in Anasarca.*

By Dr. PAUTIER, of Aigre (Charante).

Gazette Hebdomadaire; Journal of Practical Medicine and Surgery, October, 1866.)

*On the 27th of February, in a case of anasarca under his care, M. Pautier found that the symptoms had acquired the highest possible

degree of intensity. The abdominal parietes overlapped on either side the upper third of the thighs; the skin was dry, shining, and here and there covered with blisters; the dyspnœa was considerable, the voice extinct, and the pulse small and quick; a bed-sore had formed on the back, and double hydrothorax was present.

No albumen was detected in the urine.

Sudorifies, diuretics, and aperients were exhibited without any apparent benefit up to March the 9th, when M. Pautier prescribed the following treatment:—

Three cups of milk-porridge to be taken daily, each followed by the ingestion of dry bread and raw onions, without any drink.

For thirty days this diet was persevered in, and in the course of a fortnight the patient was enabled to leave his bed. In April nothing remained but slight cedema of the feet and ankles. A generous diet was then prescribed, and in another month a complete cure was effected.

ART. 69.—*On the Treatment of Chronic Dysentery.*

By Mr. HARRY LEACH, Resident Medical Officer of the
“Dreadnought” Hospital Ship.

(*The Medical Press and Circular*, September 5, 1866.)

Mr. Leach states that no disease in the practice of the “Dreadnought” hospital-ship has been treated with so great a variety of drugs as chronic dysentery. The number of remedies is a measure of the obstinacy of the malady, and the ineffectiveness of the treatment usually adopted in this malady. Depressing, however, as is this result the physician is not altogether helpless, and at times he sees his efforts crowned with success. The course of treatment recommended by Mr. Leach, from his large experience, is strictly based upon the pathology of the disease. He says:—

“The pathology of chronic dysentery is, in influencing treatment, chiefly confined to the simple fact that ulcers in variable numbers, and equally variable stages, exist along the course of the large intestine. They may be large, few in number, and may exist only near the caput coli. They may be many in number and small; and if this be the case, they are generally scattered pretty equally over the whole course of the large intestine. They may penetrate only the mucous coat of the bowel, or may burrow so deeply through the middle and into the external coat as to produce an hourly risk of perforation, and consequent peritonitis. But be the numbers and state of the ulcers what they may, putting minute and impractical considerations aside, it is clear that our endeavours must be mainly, if not wholly, directed to the giving to these ulcers a favourable opportunity of healing and cicatrization Having regard to the very large number of cases admitted into this hospital, and being sure, from personal observation, that drugs have had a fair and impartial trial, I am compelled to arrive at the conclusion

that in cases of chronic dysentery (if the disease be of more than three months' standing) very little specific good can be done by any of the so-called remedies commonly used. The only drug that can be said to have produced a definite amount of relief in any number of cases is the compound powder of ipecacuanha, and I am by no means prepared to give a very decided opinion about its efficacy. A dose of castor-oil and laudanum, given occasionally, often assists the evacuations, and affords relief from tenesmus. Beyond these, the extent of my belief in drugs no further goes. And hence it may be said, and reasonably maintained, that, as chronic dysentery may recede from the path of recovery by meddling and muddling medicine, so we may medically enjoin and employ rest in these cases as confidently and as happily as does Mr. Hilton in 'Accidents and Surgical Disease.' And rest should mean not only absolute quiet for the body, but for the bowel also. The influence of this single condition is marvellously shown in many cases admitted to the 'Dreadnought,' the progress of which cannot but lead us to refrain from meddling with the accomplishment of a process that simple quietude has evidently caused to begin. In analysing a list of fatal cases, it is found that half die from exhaustion, no other lesion except extensive ulceration along the colon being discoverable. The other moiety of cases is hastened to an end by complications of bronchitis or cirrhosis, hepatic abscess, or peritonitis. As therefore our efforts should be directed to the sustentation of the system until nature has accomplished the healing process, it is particularly necessary that care should be used in the choice and administration of diet. Milk, beef-tea, and eggs, in small and oft-repeated portions, should form the staple of the patient's food for some weeks after admission, varied occasionally by rice, arrow-root, or any other good invalid cereal. Strict quietude in the horizontal position cannot be too strongly insisted upon. If much pain exist, it may be relieved (and that without the use of opium) by light and warm applications to the abdomen. The use of stimulants is, I believe, still an open question among most practitioners; but experiences gained at this hospital lead to the conclusion that they should, as a rule, be entirely avoided. An extra allowance of good beef-tea will do far more to sustain the patient than wine or brandy, howsoever given, and I am sure that in many cases the latter are positively injurious. A placebo must, of course, be prescribed, for as there are few, if any, cases so tedious, so none require more encouragement by all the arts that influence the mind in the conservation of the body. A month, or two, or three, may elapse, with little, if any, signs of change for the better. In many cases a fatal result disappoints our best expectations; but they who, having lingered the longest, at length end well, are those with whom therapeutics have had little or nothing to do. The most favourable results can, of course, yield only a condition of comparative health, which any imprudence as to change of clothing or diet will speedily disturb and destroy. But when men, after months of probationary slop-food, reach successfully to the stage of beef and mutton diet, we discharge them as convalescents, with a good hope to them of a renewal of the lease of life. Good and gentle nursing is of paramount importance, for the vital powers are so feeble, that any risk of bed-sores must be most scrupulously avoided, and would almost invariably precede a fatal result.

“The tendency of the foregoing remarks may be condemned as too totally abandoning all therapeutic aids, but they will effect the purpose of the writer, if only they assist to propagate a plan of treatment that will allow cases of chronic dysentery to be cleverly nursed and fed, rather than actively stimulated and physicked.”

ART. 70.—*Case of Gastric Ulcer treated by Hypodermic Injection—Recovery.*

By GEORGE WILLIS, M.D.

(*Medical Times and Gazette*, July 7, 1866.)

Six years ago, C. L., then aged fifty, stated to Dr. Willis that he had suffered all his life at times from dyspepsia, which was always relieved by a little suitable treatment; that he had never been confined to his bed until Jan. 7th of that year for a single day. On that day he was seized with terrible pain in the stomach and vomiting, which state of things lasted for three months, and kept him in bed.

In the March following he had a hæmorrhage from the stomach of about two quarts, and for fourteen days after this he was treated by beef-tea and wine injections. On April 23rd Dr. Willis first saw the case, which he at once recognised as one of gastric ulcer. So great was the irritability of the stomach at that time, and so acute the agony, that not even milk and lime-water in the smallest quantities could be borne or retained. Dr. Willis tried large doses of opium, creosote, bismuth, glycerine, kino, &c.—all were rejected almost as soon as swallowed.

In this state the patient was seen in consultation by Dr. William Willis (late of the Middlesex Hospital, but now in Japan), and it was decided to use morphia hypodermically, to soothe his transit from this world rather than in the hope of cure. He had beef-tea injections, with eggs and brandy; the morphia injection eased his pain and induced sleep.

A trial was now made at the end of a week of a little milk, and it was found that a cupful would keep down and not cause very great pain if preceded by the morphia injection, which was of a strength equivalent to three grains of the salt.

This treatment was continued daily for above a year, and the diet was bread, milk, gruel, and occasionally a little fish. At the end of a year the man left his bed and came daily to our Dispensary for his treatment, which was now reduced to two grains; so cadaverous was his aspect, that people turned round in the street to look at the almost corpse-like man feebly moving along. He used to say, in his Staffordshire accent, when holding out his skinny fore-arm for the injection, “I am like a babby wanting his mammy.”

At this time he began the use of beef-tea and mutton-broth, but the pain and vomiting invariably returned if by any chance the injection was unduly delayed. For another year he gradually improved in health—his digestion became better. At the end of this year the daily hypodermic injection was only the equivalent of one grain of morphia, and

six months later he was able to do without it altogether, but took especial care in his diet. He had so far recovered in strength, looks, and flesh, that a few months afterwards he got employment at his old work, and reported of himself the other day that he had enjoyed perfect health for more than three years; that he could eat any sort of food and in full quantities; usually ate beef twice a day, and took two or three pints of beer. He has grown very ruddy, weighing nearly twelve stone, though when he left his bed he hardly weighed eight.

The only medicine taken by the mouth during his illness was a dose of salts and magnesia once a week, for without this his bowels never acted. On a few occasions an injection of atropine was substituted for the morphia in a moderately large dose, and then it never relieved pain. On one occasion the fourth of a grain produced very alarming symptoms, and was never repeated. During twenty years Dr. Willis states that he never saw a more satisfactory case or one that brought more credit to treatment, and he hopes that such a valuable mitigation of suffering may never fall into disuse. He adds that it is now the only solace of a man dying of cancer of the rectum, and he requires six grains of morphia daily.

ART. 71.—*A Case of Fatal Peritonitis from Perforation of the Appendix Vermiformis.*

By Dr. A. D. HALL and Dr. CORSE.

(*American Journal of the Medical Sciences*, October, 1866.)

Mrs. S., aged twenty-five years, of slender frame and rather delicate health, the mother of two children, the youngest four months old, both of whom she had been obliged to wean soon after birth, from want of nourishment, had complained for one or two weeks chiefly of languor and debility, when she was attacked, on the 10th of March, with severe pain in the lower part of the abdomen, followed by nausea and vomiting. The pain continued during the two following days, but had somewhat abated on Tuesday, the 13th, when she ate a piece of chicken, after which it became greatly aggravated and the vomiting still more incessant. On Wednesday, the case having assumed a more serious aspect, her husband, himself a physician, sought other counsel. At this time her pulse was 140, abdomen extremely tender on pressure, bowels obstinately constipated and somewhat tympanitic, and the stomach extremely irritable. She lay upon her side, with her legs drawn up, and had an anxious, distressed countenance. There was no tumour or other indication of hernia to account for the symptoms, and the disease was at once recognised as peritonitis of the most general and dangerous type. The usual remedies for peritonitis were employed without effect, and the patient died on the 15th of March.

Inspection, fifteen hours after Death.—The body preserved in ice; rather a small, spare figure, face thin, surface of abdomen marked with leech-bites and the effects of a blister. Small amount of fat in the abdominal parietes. Half an ounce of fluid pus was found in the inter-

space between the stomach and liver. The folds of the mesentery and the convolutions of the intestines were glued together by easily separated lymph. About six ounces of brownish turbid serum were found in the pelvic cavity. The results of inflammation appeared, however, most marked in the right iliac region, and hence arose a suspicion of perforation of the intestine in that neighbourhood. On a careful examination, a perforating ulcer of the appendix was found, through which a grooved director could be passed, communicating freely with the peritoneal cavity. The perforations were two in number. The first was one inch from the caput coli; the muscular coat of the bowel appeared to have been destroyed by ulceration, and then the peritoneal coat had given way in three small openings one-sixteenth of an inch in diameter; these were arranged in a triangular manner. The second was a solitary perforation an inch and a half from the end of the appendicula. The mucous membrane of the process appeared to be extensively ulcerated, and in places presented the cribriform appearance not unfrequently found in the intestinal tract of patients dying of Bright's disease. Although thick patches of lymph had been thrown out in separate deposits, still no attempt by nature to limit the effusion of foreign material by lymph barriers was discoverable. There was nothing to show that any foreign body nor that any impaction of feces had been the origin of the lesion, although the material, if such had been the origin of the mischief, might readily enough have escaped into the cavity of the abdomen.

ART. 72.—*On the Treatment of Infantile Diarrhœa.*

By Dr. BUIZ.

(*Journal des Connaissances Medicales; Journal of Practical Medicine and Surgery*, August, 1866.)

Dr. Buiz expresses the following opinions, as the results of his experience, on this subject:—

“1. The diarrhœa of spoon-fed infants generally yields to the addition of a small quantity of bicarbonate of soda or of lime-water to the milk.

“2. In summer-diarrhœa supervening without any tangible cause, from one-sixth to one-quarter of a grain of calomel three or four times a day, associated with an equal amount of ipecacuanha will often be found efficacious. If the indisposition is consequent on exposure to cold, minute doses of opium are appropriate.

“3. Chronic diarrhœa resulting from various causes may in most cases be checked with nitrate of silver, one-sixth of a grain of which may be exhibited without risk. This remedy is sometimes, however, rejected by the stomach, and should then be replaced by tonics and vegetable astringents.

“4. Diarrhœa combined with anæmia and impaired nutrition, is often the result of a state of decomposition of the blood, for which the best remedy is the proto-iodide of iron. In such cases bismuth is fre-

quently unavailing, whereas in doses of half a drachm three times a day it is invariably successful against intestinal relaxations referable to tubercular ulceration. The causes of intestinal catarrh are, however, so obscure, that in many instances the treatment must be empirical."

ART. 73.—*A Case of Hydatid of the Liver, treated successfully by the Injection of the Extract of Male Fern into the Cyst.*

By Dr. F. W. PAVY, Assistant-Physician, Guy's Hospital.

(*Proceedings of Royal Medico-Chirurgical Society*, 1866 ; *Medical Times and Gazette*, September 29, 1866.)

Harriet V., a woman of pretty healthy appearance, aged twenty-one, admitted into Mary Ward, Guy's Hospital, under the care of Dr. Pavy, Oct. 4th, 1865. When three years old she was squeezed against a wall by a cart-wheel, which struck her on the right side of the chest. No rib was fractured, and she soon recovered from the accident. About six years ago the patient noticed a slight swelling in her right side, which has since continued gradually increasing in size. On examination, a large deep-seated tumour was to be noticed occupying the right hypochondriac region, and extending considerably beyond, both above and below. Its boundary could be clearly defined inferiorly. It caused a considerable bulging of the ribs on the right side, and the right mammary gland was raised about three quarters of an inch above the level of the left. Fluctuation was apparent. Dulness extended as high as the lower border of the second rib on the right side. The case was diagnosed to be one of hydatid tumour of the liver. The relationship that is agreed upon by helminthologists to exist between the hydatid and the tænia, and the known effect of the extract of male fern upon the latter, suggested to the author the treatment adopted. The extract is not miscible with alcohol or water, but it was ascertained that a liquid sufficiently thin for passing through a fine canula was to be obtained by admixture with a little potash. November 6th.—A fine trocar and canula were introduced into the tumour by Mr. Durham, and about four ounces of a limpid colourless fluid allowed to escape, in order to diminish the tension of the cyst. A liquid consisting of half a drachm (by measure) of the purified semi-fluid extract of male fern, half a drachm of liquor potassæ, and six drachms of water, was then injected into the sac, care being taken throughout to prevent the entrance of air. The fluid removed was examined, and found to be non-albuminous, charged with a large quantity of the chloride of sodium, and to contain hooklets of the echinococcus. At the introduction of the trocar the patient complained of experiencing a considerable amount of pain, which she referred to the lower part of the abdomen. Some febrile excitement, vomiting, and purging followed, but there was no evidence of peritonitis. 10th.—On percussion, it was found that dulness did not extend so high in the chest on the right side by one rib as previous to the operation. 16th.—The patient was allowed to get up. 20th.—

The tumour was found to be much diminished in size. It was much softer, did not extend so low down in the abdomen, and was much less distinctly circumscribed. The chest was resonant on percussion as low as the space between the fourth and fifth ribs. 29th.—The circumference over the most projecting part of the tumour before the operation was $34\frac{1}{4}$ inches; to-day it is $31\frac{3}{4}$ inches, showing a reduction of $2\frac{1}{2}$ inches. Tumour very soft, and its lower border not to be defined as formerly. The patient, being well, was allowed to leave the hospital. A fortnight, and again a month, afterwards she was seen, and found to be progressing satisfactorily. May 10th, 1866.—Since she was last seen the patient had suffered from an attack of rheumatic fever with heart complication and bronchitis. She had been in no way troubled with her side, and her circumference now was 30 inches. No swelling was perceptible to the eye, but a hardness remained in the hypochondriac region. The inference to be drawn from the result in this case is, that the injection of the extract of male fern caused an immediate destruction of the life of the hydatid without the production of suppuration, and that a rapid absorption of the fluid element of the cyst afterwards took place.

(E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 74.—*Remarks on Chronic Albuminuria, Originating during the Convalescence from Scarlet Fever and other Eruptive Diseases.*

By Dr. HERMANN WEBER, Physician to the German Hospital.

(*Proceedings of the Royal Medico-Chirurgical Society, 1866; Medical Times and Gazette, September 29, 1866.*)

These remarks do not refer to the well-known and easily-recognised acute scarlatinal dropsy or desquamative nephritis of scarlet fever, but to a chronic form of albuminuria originating occasionally at a much later period, when recovery had apparently been established already for several weeks. The author related three cases of scarlet fever, unattended with albumen in the urine or any other symptoms of renal complication during the first four weeks from the commencement. The subjects of the cases appeared quite well at the end of about a month; when they returned to their usual mode of living; but about three or four weeks later the general health became disturbed (loss of appetite and strength, glandular swellings, boils, anæmia, and occasional sickness), and the urine, as soon as the patients came under treatment, was found highly albuminous. Perfect recovery took place in one case; while in another the general health became much improved, but a slight degree of albuminuria has remained; and in the third case death occurred seven years after the commencement from broncho-pneumonia, with uræmic symptoms, the *post-mortem* examination exhibiting waxy degeneration (amyloid) of the kidneys. The author maintains that the connexion between the scarlet fever and the renal disease in this class of

cases is not the same as in the acute scarlatinal dropsy; while the latter may be considered as a part of the scarlet-fever process, the former, originating at a much later period, is probably only so far connected with the scarlet fever that through it a greater susceptibility to the development of chronic renal disease is effected, in the same way as there results a tendency to other chronic affections, like glandular swellings and eruption of boils. The author believes that the same tendency may be caused also by other acute diseases, especially those of exanthematous nature, and he gave two cases in which chronic albuminuria took its origin in persons who had lately recovered from erysipelas of the head and typhoid fever respectively, in both of which cases, during the febrile state and during the convalescence, the urine had been quite free from albumen. He referred also to a similar case occurring after typhus fever, and described by Dr. Johnson in his work on *Diseases of the Kidneys* (London, 1852, p. 408). The author was inclined to infer from such cases that amongst the many cases of Bright's disease the origin of which is uncertain, a not inconsiderable proportion may have been developed during the later stages of convalescence from exanthematous diseases; that, therefore, as urged already by Dr. Johnson with regard to fever, particular care ought to be taken during those stages with regard to diet, clothing, habitation, avoidance of over-exertion and exposure to cold and damp air. Dr. Weber further pointed out the insidiousness of the commencement of the chronic albuminuria, as, in four cases out of the five related, anasarca and admixture of blood with the urine were altogether absent. Lassitude, loss of strength, anorexia, swelling of the lymphatic glands, and eruptions of boils, being the principal symptoms, ought, therefore, always to lead to an examination of the urine, the more so as by an early discovery of the renal disease the chance of a perfect cure is much increased, as seen in two of the five cases reported.

The treatment consisted in attention to skin and diet; in the administration of iron with acidulated acetate of ammonia, and occasional doses of elaterium to relieve the kidneys, and in the use of the hot vapour bath or the warm wet sheet.

ART. 75.—*On Paroxysmal Hæmaturia.*

By Dr. F. W. PAVY, F.R.S., Assistant-Physician and Lecturer on Physiology, Guy's Hospital.

(*The Lancet*, July 14, 1866.)

Dr. Pavy reports the following cases of a form of hæmaturia, which he denominates paroxysmal. He considers that in these cases an unusual susceptibility of the kidney to temporary congestion from exposure to cold existed:—

“A middle-aged gentleman residing in Suffolk, came to consult me in November, 1864, bringing with him a specimen of urine which he

had passed on the previous day. He informed me that a slight exposure to cold brought on a fit of shivering, which was followed by the passage of urine of a more or less porter-like colour; and that, usually by the next day, the natural appearance of it became restored. The specimen of urine he brought had been passed some time after a paroxysm of shivering, which had come on from driving in an open vehicle on the previous day. It was high-coloured, but not dark like porter; opaque from the presence of lithates; and, after the lithates cleared up, on the application of heat, a deposit of albumen came down. On examining it microscopically, a large number of octahedral, oxalate-of-lime crystals were to be seen with the amorphous deposit of lithates. The urine he passed at my request during his visit was natural in colour and entirely free from albumen. A fortnight later I saw him again. He had suffered from no fresh attack, and, beyond a deposit of crystals of lithic acid, his urine was perfectly natural. A fortnight after this he brought me a specimen of very dark-coloured urine, which he had voided shortly after a shivering fit. This I found to be highly charged with albumen; and, by the microscope, casts of tubules, some blood-corpuscles, coloured granules, and numerous crystals of lithic acid were brought into view. The urine of the following day was natural in colour, and only slightly albuminous; and on the day after this the urine had completely resumed a natural character.

“My patient informed me that during the last year or two he had frequently suffered from similar attacks; that he had been under different medical men, and had taken quinine and various other remedies for his complaint. It had been ascertained that there was no stone in his bladder; and it was not exercise but exposure to cold that brought on his attacks. He was comparatively free from them in the summer. There was no periodicity about their recurrence of the character belonging to ague; and he had never, as far as he knew, been the subject of that complaint. Riding or driving on a cold day, if his feet got cold, sufficed to bring them on; and on this account he had been obliged to give up riding with the harriers, a sport he had been hitherto frequently accustomed to join in. Going out of doors and standing for a few minutes in the cold air, although well clad, had been sufficient to bring on an attack. If he could only keep his hands and feet warm, he told me, he felt safe; but as soon as these parts got cold he was almost sure to have an attack. His countenance was somewhat sallow, and he had been losing strength and flesh.

“With this history, I prescribed a tonic and some extract of belladonna, and urged him to wear fur gloves and over-boots when he went out of doors in the cold. Adopting this plan he passed through the remainder of the winter of 1864-5 almost free from attacks, and improved in health and strength. If such had not taken place, I had determined to advise him to change his residence during the winter to a warmer climate; but this was, if possible, to be avoided, as it would have involved with him a considerable pecuniary sacrifice.

“During the past winter he has got about much more freely, and in the early part of it was almost free from attacks. In the latter part of it, however, he had been less careful of himself, and had suffered accordingly. Sometimes he has averted an attack by going in-

doors directly he felt it coming on, sitting in front of a fire, and drinking something warm. In April last he wrote to me as follows:—

“My last attack was brought on by riding in the cold, although I did not feel it but very little until I got to the end of my journey, was sitting in a warm room, and had a slight shivering fit. Had a glass of hot sherry-and-water, and in about ten minutes I wanted to make water. Did so, and it was the colour of porter. In about ten minutes again, the same irritation as before, and made about a wineglass of water, more blood colour, and the same in about another quarter of an hour. Then I became very warm and feverish, and I did not make water again for more than two hours; when I did so it was quite clear. On Tuesday morning last, when I got up at half-past eight, I washed in cold water, and sat down to breakfast. My feet turned cold, and I felt a little queer, so I turned to the fire, and drank a cup of hot tea, and got warm. When I made water about two hours after, it was the colour of dark ale, not quite so dark as porter. I always use myself to wash in warm water. I still have the cough when attacked, and until the perspiration comes on it does not leave me. After this I feel faint, and sometimes almost faint off. I always feel cold at the chest before passing blood.”

“The other case is that of a gentleman, rather beyond the middle period of life, who was brought to me by Mr. Acton in December last. The urine passed by the patient at my house was natural in colour, and free from albumen; but I was informed that it was occasionally for one or two micturitions highly charged with blood. Mr. Acton had several times seen it in this state. The history disclosed the same connexion between the attack of hæmaturia and exposure to cold which existed in my other patient, and I expressed the opinion that I considered it to be a perfectly parallel case.”

ART. 76.—*On a singular Case in which a large amount of Iodide of Potassium was excreted in the Urine, with subsequent Disappearance of the Glucosuria which existed at the time.*

By JAMES BRAITHWAITE, M.D. Lond., Leeds.

(*British Medical Journal*, September 8, 1866.)

Dr. James Braithwaite records the following curious case:—

“On the 2nd of December, 1864, I was sent for to attend a woman sixty-four years of age, who complained of feverish symptoms, which she attributed to cold. The skin was hot, the pulse quick; and there were mucous *râles* all over the posterior part of the chest. She had cough, and expectoration, which, however, was not rusty in appearance. Her urine was free both from sugar and albumen. She soon improved, and went down stairs again, but she did not regain her strength beyond a certain point, and during the early part of January she grew weaker; complained very much of the cold; her skin assumed a dusky shrivelled appearance; her appetite entirely left her; and she was obliged to return to bed. She was quite free from fever, but so feeble that she

could hardly remain upright in a chair many minutes at a time. I found that her urine now contained sugar; it was of specific gravity varying from 1022 to 1032; it was neutral or slightly alkaline immediately secreted; it contained a mere trace of uric acid. This was the state of the urine on the 18th, 19th, and 20th of January. On the 21st, however, the sugar had quite disappeared—there was no trace of it; but, on adding strong nitric acid to the cold urine, a dense and very copious, reddish black, amorphous precipitate was thrown down. This consisted of pure iodine, which was precipitated in a crystalline form by adding nitric acid to the urine, and which gave the characteristic blue reaction with starch.

“On the next day, the sugar again reappeared in the urine in large amount; the specific gravity was 1032; but there was not a trace of iodine.

“On the 23rd, the specific gravity was 1041; and there were both iodine and sugar. The following day, there was the sugar alone; nor did any iodine subsequently appear in the fortnight during which I daily examined the urine; but the sugar remained as before. From this time she rapidly regained her health and strength, and I ceased to attend her. On May 1st, I found the urine quite free from sugar; and I have recently found it so still.

“It is difficult to say, with any approach to accuracy, what amount of iodine was passed in the two days on which it occurred in the urine. I am satisfied, however, that it was very large; for the precipitate from four ounces of urine covered the bottom of the porcelain dish, in which it was just contained, to a depth of rather more than a line. I unfortunately did not weigh the precipitate obtained from four ounces of the urine; but it is, I think, under the mark to say that it would fill a drachm measure, and would consequently weigh about twelve grains.

“I found that, three years before, she had taken iodide of potassium for rheumatic pains. This she procured at a druggist's, as she wanted it, in small quantities at a time. She has no idea how much she took. At the time she was passing sugar in the urine, she had none of the usual thirst of diabetes; nor was the urine much increased in amount. At first it could not be measured, owing to her having a little diarrhoea, and her voiding urine at the same time; but later in the case, when sugar was still present, I found the amount was about forty ounces. It may be objected, that the patient took the iodide on the days on which it was found in the urine; but I think that the large amount, and the fact of its occurrence on two days only, and these days separated by one during which no iodide was passed, preclude this idea. Ten or twelve days afterwards, I taxed her with taking iodide of potassium unknown to me. She most emphatically denied it, but told me at once that she had taken it three years before. The fact of the retention of so soluble a salt, and one which is generally eliminated so readily in the urine, for so long a period, is interesting in itself, and especially so in reference to the temporary diabetes produced, which, however, lasted at least two months. That the salt seemed to be set free by the previous attack of fever, may be explained on the supposition that it was retained owing to the electric affinity with some organ or organs, which affinity was destroyed, owing to a change in the polar state from fever. It is

well ascertained, that the blood is always electro-positive with regard to the secreted fluids, which are electro-negative. It is possible that in fever the electro-positive state of the blood may be altered or lessened, and that this may account for the diminished secretions."

ART. 77.—*A Case of Single Kidney (Right).*

By DR. JOHN C. MURRAY, Newcastle-on-Tyne.

(*British Medical Journal*, August 11, 1866.)

E. F., a strong muscular man, of florid complexion, aged sixty-five years, five feet nine inches in height, born in the seventh month of utero-gestation, had always passed less urine, but of a deeper colour, than normal. Nevertheless, he enjoyed good health until 1846 (his forty-sixth year); he then first suffered from nephralgic pain in his back, which lanced down the course of the right ureter to the testicle and thigh of the same side, but ceased on his being cupped. From that time, after fatigue and exposure, he frequently felt pain in the right lumbar region, accompanied by dysuria, and sometimes hæmaturia; but never any similar pain in the left side. He was a free, but regular liver; had been a blacksmith until 1858, when he received a Government appointment, which had the effect of suddenly increasing his weight from $10\frac{1}{2}$ to 13 stone, at which weight he remained, without variation, until his death.

On the 27th of May, 1866, he was seized with enteritic symptoms in the right iliac fossa, but continued his employment until the 30th, when he was obliged to relinquish duty and obtain professional aid. Dr. Murray was sent for, bled him, and administered appropriate remedies; after which he did very well until the 3rd of June. An unfavourable change having occurred on that day, Dr. Murray saw him, in conjunction with Dr. White; but the patient was then so low that their efforts were fruitless to save him. He sank rapidly, and died on the morning of June 4th. At this untoward event Dr. Murray was much surprised, as he had been progressing so favourably. He, however, attributes it to the patient's solitary kidney becoming, from its close relationship to the inflamed colon, too congested for the continuance of its functions.

Twenty-five hours after death, Mr. James Douglas Murray and Dr. Murray found *post mortem* appearances of enteritis, which had evidently commenced at the caput cæcum coli, and extended along the ascending and part of the transverse colon. The rest of the bowel was in its natural state. The liver was not *fatty*, or otherwised diseased. Upon taking out the right kidney for inspection, they were astonished at its unusual size and weight; but, thinking it only enlarged from recent fatty degeneration, Dr. Murray made an incision from its convex border to the hilum, to see its internal structure; and also cut off a thin slice for subsequent examination. They then proceeded to inspect the left lumbar region, for the purpose of comparing the right kidney with its fellow. In this, however, they were disappointed. No trace of a left

kidney, collapsed, atrophied, or yet rudimentary, existed; nor was there any semblance to renal vessels. The kidneys being occasionally variable in their relations, and often mobile, the whole of the abdominal cavity, and even pelvis was carefully examined; but the left kidney was wanting. Attention was then directed to the right kidney with increased interest. It was in its natural site, immobile, and deeply imbedded in fat. It was of normal form, exaggerated in all its parts, somewhat firmer and less friable in texture than natural. It showed some fatty deposit in its cortical part, but not enough to materially impair its functions. This, Dr. Murray thinks, may be accounted for by hyperæmia, consequent upon its having double duty to perform. It was deeply injected where in relation to the ascending colon, the congestion penetrating its entire diameter. Part of the surface was of its natural colour; and its original division into three lobes was perceptible. The investing tunic peeled readily off. After the kidney had been carefully washed and pressed, its proportions and weight, allowing for the quarter of an ounce cut off, were:—

Length, 6 inches;		Diameter, $2\frac{1}{2}$ inches;
Breadth, $3\frac{5}{8}$ inches;		Weight, $10\frac{1}{4}$ ounces.

ART. 78.—*On the Treatment of Addison's Disease.*

By Dr. E. HEADLAM GREENHOW, Consulting Physician to the Western General Dispensary, Assistant-Physician to the Middlesex Hospital.

(*British Medical Journal*, July 14, 1866.)

In a clinical lecture on this subject, Dr. Greenhow observes, that it seems to him unquestionable, from the history especially of a case he details, that Addison's disease, although incapable of cure, is yet in some degree amenable to treatment in respect of delaying its progress, unless the illness has already arrived at its later stages. Owing, no doubt, greatly to our still imperfect knowledge of the nature and causes of the disease, the means of treatment at our disposal are as yet unfortunately scanty. The remarkable asthenia, however, by which the disease is characterized, the constant tendency of the patients to succumb under any powerful depressing influence, and the strong evidence as to the disease being frequently a result of surrounding irritation, are facts which clearly indicate the necessity for tonic treatment and nutritive diet, the avoidance of all causes of depression, and the great value of rest and of such therapeutic agents as may relieve the vomiting and other exhausting symptoms, and tend to invigorate the general health. Prolonged rest in bed, and subsequent avoidance of fatigue, or indeed of much bodily exertion or mental strain of any kind, have formed essential parts of the management in all the cases which have improved for a time under his observation. The use of drastic purgatives should also be scrupulously avoided in these cases. Consti-

pation is more common than otherwise in Addison's disease ; but, unless it be very extreme, Dr. Greenhow thinks it better to abstain from interference than to risk the dangerous depression which often follows the administration of aperient medicines ; and he instances the case of a young girl who died under his care of Addison's disease somewhat more than a year ago, and whose fatal seizure appeared to have been brought on by the effects of a dose of calomel and jalap given her by her mother.

As regards diet, the only plan is to give nourishing food of whatever kind the patient's stomach can best bear, and this will probably vary more or less in every case ; substituting milk, eggs, jellies, oysters, and the like, for the stronger diet of meat or soups, when the stomach cannot tolerate these latter.

For the relief of the nausea and vomiting, ice, soda-water and brandy, chloroform or creasote, bismuth and effervescing medicines, with citrate of iron, have each in turn proved useful in his hands ; and, again, each at times has failed to effect any good purpose. After the sickness has abated, decided benefit sometimes attends the administration of chalybeates and cod-liver oil or glycerine. In two cases he records, cod-liver oil disagreed with the patients ; but glycerine, in doses of two drachms, combined with fifteen or twenty minims each of spirit of chloroform and of the tincture of sesquichloride of iron of the *London Pharmacopæia*, has been of great service. Dr. Greenhow speaks positively on this point, because in each case the patients on several occasions having discontinued the medicine as soon as they felt better for it, have then fallen off, and on applying to him and resuming its use, have in a week or ten days began to improve again without any other simultaneous change in their treatment or circumstances.

ART. 79.—*On the Treatment of Hepatic Gravel.*

By Dr. SUTTON.

(*Journal of Practical Medicine and Surgery*, May, 1866.)

The curative measures applicable to calculous disease of the liver should be instituted only in the intervals of the attacks of hepatic colic, after all symptoms of local irritation have subsided. Any other course would aggravate symptoms which each require separate attention.

In order to introduce a certain method into the description of the remedial measures applicable in this disease, Dr. Sutton, in the first place, adverts to the treatment of biliary calculi ; and, in the second, to the management of the symptoms induced by their presence.

Solvents are the first class of remedies resorted to at all periods.

Alkaline solvents should be preferred to all others. They have effected lasting cures, and sometimes disaggregate the concretions, or cause them to be passed with copious discharges of bile. This crisis is often preceded by severe hepatic colic, induced by the treatment, and it may not be without peril.

Alkaline treatment includes various substances, such as the fixed alkalies, soap-boilers' lye, soda, carbonate of ammonia, medicinal soap, alkaline salts with vegetable acids, tartrates, citrates, &c. The remedy most commonly resorted to is the exhibition of the mineral waters of Vichy, Vals, Carlsbad, Ems, &c. These waters should also be used in baths, and they should be persevered in, at intervals, for several successive years.

Durande's remedy consists in the daily administration of half a teaspoonful or one teaspoonful of the following mixture:—

R. *Ætheris sulphur.* ʒiv.
Ol. terebinth. ʒijss.

This medicine has occasionally been serviceable, but its good effects are not due to its solvent power, the concretions being generally passed with the motions, and the mixture deserves, therefore, to be classed not with solvents, but with evacuants.

Chloroform has also been highly extolled; but its efficacy is very doubtful, and its utility is perhaps confined to its sedative influence.

Aperients are commendable as mechanical agents of expulsion. Frictions, shower-baths, shampooing, and electricity have frequently been resorted to for the purpose of promoting the escape of the calculi. But the methodical exhibition of purgatives, such as castor-oil or sulphate of soda, is far more reliable.

The diet should consist of laxative vegetables, fresh herbs, succory, borage, liverwort, &c.; grapes, acid fruit, and whey are also beneficial.

The use of all greasy substances whatever should be strictly prohibited. Light food, roast or boiled, feculents, lemonade, are appropriate; and exercise should be enjoined less with a view to the increased combustion of fatty matter, than to promote the flow of the bile into the intestine, and thus prevent its stagnation in the gall bladder.

In the treatment of hepatic colic, it is desirable to allay the excruciating pain if possible; opium, even as much as three or four grains, may be fearlessly prescribed; also subcutaneous injections of muriate of morphia, which are more efficacious than the internal exhibition of the salt.

Belladonna, much recommended by Bretonneau and Lalotte, is less reliable than opium, and should only be used when this remedy is not available. The same remark applies to the *aq. dest. lauro-cerasi*, and to the *tincture of castoreum*.

The inhalation of chloroform is an invaluable resource, when the paroxysms are intense; it not only acts as a sedative of pain, but the collapse which follows anæsthesia is highly favourable to the cessation of the spasmodic contraction of the biliary passages, and to the ultimate escape of the concretions.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 80.—*On the Use of Chloride of Iron in certain Cutaneous Diseases.*

By Dr. BEDFORD BROWN, of Washington City, D.C.

(American Journal of the Medical Sciences, April, 1866.)

Dr. Brown is desirous to direct attention to the value of chloride of iron in the treatment of certain acute and chronic affections of the skin. He makes the following observations on the special application of the remedy:—

“Action of Chloride of Iron in Confluent and Malignant Forms of Variola.—As a general thing, though not always, the extent of cutaneous inflammation and suppuration, arising during the progress of smallpox, denote the gravity of type. Hence, in the management of this affection, the conditions of the skin becomes the absorbing consideration.

“During an extensive experience in epidemic smallpox, from previous knowledge of the peculiar action of chlor. ferri in diffuse or erysipelatous inflammations, I was induced to test the powers of the remedy in the former affection, on the principle of its influence to control, curtail, or diminish such forms of inflammation. And now, after having fairly tested the value of this remedy in the grave and malignant forms of small-pox, I am prepared to give my testimony in its favour.

“The action of the remedy in all the cases tested was gradual, but apparent and decided; simultaneously modifying and diminishing inflammatory action, and curtailing the process of suppuration, as it were confining these processes to safe limits. Of its effects, not the least valuable in this disease is that on the tedious and exhausting suppuration attending it. For the amelioration of this symptom the chloride of iron, in my own experience, is incomparably superior to all other means. And here I will take the liberty of digressing from the main subject, for the purpose of stating a case in testimony of the fact.

“A youth of sixteen had sustained an enormous burn of the right lower extremity, from the lumbar region to the toes, which caused the entire skin of the injured parts to slough, leaving the fascia at some points, and the muscles at others, entirely exposed. When I saw him, three months after the reception of the injury, the denuded surface was covered with enormous masses of granulation, from which flowed forth incredible quantities of purulent matter daily. The patient was suffering from extreme exhaustion and emaciation, and was far advanced in hectic. Large and frequently repeated doses of chloride of iron were prescribed in connexion with generous diet, I believe as much as \mathfrak{ss} . every five hours, during the first week. Under this system of treatment the improvement was decided and progressive, and finally ended in complete recovery; and, to my utmost astonishment, there was but little thickening or cicatrization of the skin left, where before there were enormous and unsightly granulations, filled with engorged capillaries.

“To return to the consideration of the treatment of small-pox; the

two following cases may be stated as examples of the efficiency of the remedy.

“Mrs. P., in the seventh month of pregnancy, contracted small-pox, three of her children having it at the time. Abortion occurred during the first day of eruption. The case proved to be a most malignant confluent type of disease. The entire surface was covered with eruption, extending over the throat and vaginal surface; the face and tongue were also greatly swollen; the constitutional symptoms were violent. Having, as I now supposed, a favourable case to test the powers of the chloride of iron as an agent to control severe inflammations of the skin, I eagerly embraced this opportunity to put it in practice. The remedy was prescribed in quantities of \mathfrak{zss} . every four hours, after the operation of an aperient. Subsequently the variolous pharyngitis becoming very violent, the chlorate of potash was added. Alarming symptoms of general prostration also presenting themselves were counteracted by the free administration of stimulants.

“This method of treatment was persevered in from an early period of the eruption, with but little variation, and in the usual time the patient passed through the different morbid stages triumphantly. It has been my lot to observe but few cases of small-pox of a more alarming character than this. From the enormous amount of eruption I had apprehended copious suppuration. To the contrary, the process of maturation was greatly modified, and attending suppuration proved moderate.

“It may be claimed even for the chloride of iron in small-pox, that it possesses very considerable abortive powers. In that most alarming and grave complication, where the tongue is greatly enlarged, and the powers of articulation and deglutition are almost destroyed, this combination of remedies is valuable.

“Mrs. M., a woman aged fifty-five, contracted a most malignant attack of confluent small-pox, attended with all the usual indications of alarming adynamia, as great vital prostration, feeble and frequent pulse, cold extremities, delirium, dry tongue, and diarrhoea. She was ordered the chloride of iron in doses of forty drops every four hours, with a liberal allowance of alcoholic stimulants and nutritious diet. The system responded kindly to the action of the remedies; the type of disease soon became modified; and the patient passed through the attack with unexpected safety and rapidity. In neither of these cases did the amount of suppuration correspond with the severity of symptoms. Nor was the process of maturation fully developed. These cases are merely a representation of others of a similar character, which were subjected to identical treatment with equally favourable results.

“*Carbuncle*.—Carbuncle, though usually not classed with cutaneous affections, yet is a malady so thoroughly involving the skin and sub-jacent tissue, as to entitle it to a place among them. The principle of regulating the circulation, or curtailing or diminishing it to an inflamed point—*cr*, in other words, changing the character of *diffuse* forms of inflammation, and converting them into circumscribed forms, and establishing certain well-defined limits for their isolation by internal means possessing very active astringent or hæmostatic properties—is simply analogous in action to that of the same means when applied for the arrest of hæmorrhage. In both instances the agent is brought into immediate contact with the capillary vessels and their contents. The inflammation of carbuncle is peculiarly destructive, with a strong tendency to extend to all neighbouring parts, and with no power for the

establishment of fixed limits. Hence in treatment, the leading considerations are, to change the diffuse character; establish defined bounds, and promote moderate healthy suppuration; to enable the system to arrest disease, and to throw off sloughing material. For the promotion of these objects no remedy, in my own experience, acts so promptly, when given internally, as the chloride of iron.

“The following case may be accepted as an example:—

“Mr. H., aged fifty-five, of previous intemperate habits, was attacked with large and very dangerous carbuncle on the right shoulder, which presented a disposition to rapid extension and sloughing. The general indications were eminently typhoid.

“Chloride of iron, in quantities of \mathfrak{zss} . every four hours, was prescribed in connexion with generous diet and stimulants, with the best effects. It was not long before there was a perceptible decline in the inflammatory action, while a well-defined boundary was being clearly established between the healthy and diseased parts, and the suppurative process was being modified. A very large slough was eventually thrown out, leaving a cavity behind filled with healthy granulations. His recovery was both rapid and continuous.

“*Chronic Pustular and Vesicular Affections.*—After a very considerable experience in the use of the chloride of iron in these classes of affection, I am warranted in the expression of great confidence in its efficacy; not only for the removal of the immediate local affection, but for the correction of those prime causes of a predisposing character. In all stages, but more particularly in the advanced stage of that very annoying and loathsome affection, chronic eczema, when ulceration and suppuration are fully established, and the integument has become thickened, indurated, and laid open with numerous fissures, the chloride of iron treatment is followed by the best results.

“For the full realization of these results, time and patience are requisite; but these are always manifested by diminution of the copious discharges, reduction of cutaneous engorgement, and a better regulation of capillary circulation, and finally by a restoration of the thickened and indurated integument. For both promptness and permanence of action, it is far preferable to the preparations of arsenic, iodine, or mercury in common use. It is particularly adapted to the tender age of children, whose delicate digestive organs cannot always tolerate the latter articles. The action of the remedy is gradual in this class of affection; but in the end success will always crown our efforts, and may be calculated on almost with a certainty, even when all other means have failed.

“*Impetigo and Ecthyma.*—One of the most interesting features presented in the action of the chloride of iron is the remarkable influence which it exerts over the formation and excretion of pus, by operating through the channel of the circulation. Hence, through this power to control and regulate the process of suppuration, and suppurative inflammation, it is most admirably adapted to the treatment of this class of chronic affections, in which suppuration is a leading element. The action of the remedy in impetigo and ecthyma is decided and often speedy. Under the administration of the chloride of iron, I have not unfrequently observed the chronic ecthyma of children to disappear in

a period of ten days. It also prevents or removes another very annoying feature in this affection, tedious ulceration.

“*Phagedenic Affections of the Skin.*—In this type of disease, which are so prone to march on in their progress of destruction, regardless of treatment, the chloride of iron, in my hands, has afforded results superior to all other means. The character of the peculiar destructive ulceration as well as the inflammation becomes changed; the progress of ulceration arrested; and, finally, granulations take the place of sloughs. In that variety of phagedena appearing during the progress of syphilis, and where the preparations of mercury become offensive to the system, and cannot be employed, I find the internal use of chloride of iron and chlorate of potash combined most valuable correctives.

“Both the officinal ‘*tinctura ferri chloridi*’ and the solution in water of the salt, have been used by me, but I regard the former as the most efficacious.”

ART. 81.—*On the Treatment of Bromidrosis.*

By Dr. ARTHUR WYNNE FOOT.

(*Dublin Quarterly Journal of Medical Science*, May, 1866.)

Dr. Foot makes the following observations on this subject:—

“The treatment of fetid perspiration, when not due to the use of food or medicines, known to affect the secretion of the skin in this peculiar manner, should be both internal and external. The sudden arrest of the secretion of the skin, which is sometimes the consequence of violent measures to check bromidrosis, has been followed by neuralgia and other constitutional disturbances, whence the complaint was formerly regarded as an endeavour on the part of nature to eliminate, per cutem, a *materia peccans* with whose excretion no therapeutical interference was allowable. Like fetor of the breath, it sometimes can be traced to derangement of the stomach and digestion, which having been remedied, perspiration will return to its natural condition, but the causes being generally subtle and various, the treatment will differ with each individual case. Among the remedies most successful is arsenic, highly spoken of by Milton, who also recommends that, in the oldest cases, the hair of the arm-pits should be cut short or pulled out, and these parts, with the folds of the groins and the feet, to be washed every day with soap and hot water, and then dusted, after being dried, with rice powder; if any smell remains after this, the free use of chloride of zinc or permanganate of potash in lotion should be resorted to. Small pads stuffed with animal charcoal and secured in the arm-pit absorb and deodorize the secretions of this region; and arrangements of the inner clothing, adapted to the exigencies of the case, should be made with the view of facilitating evaporation, as the more confined the perspiration the more concentrated and powerful is the odour. M. Stanislaus Martin (*Bull. de Thérap.*, tom. lxx., p. 143) has contrived a mode of applying charcoal as a local deodorizer in fetid perspiration of the feet. A paste composed of forty parts of powdered charcoal, forty of water, and fifteen of gum, should

be thickly spread over a piece of filtering paper, flannel, or felt, stretched over a board or pasteboard; the paste is then covered over with another piece of paper, which is to be smoothed with the hand so as to remove all asperities; the whole is submitted to compression during an hour, after which the water is to be allowed to evaporate; when quite dry the sole may be cut out of the required size: the soles, being inexpensive, can be changed once or twice a day if necessary. Gaffard, of Aurillac, recommends the use of a lotion composed of fifteen grains of red oxide of lead and seven and one-half drachms of solution of acetate of lead; his directions are, to pound the oxide of lead in a porcelain mortar, add the acetate by degrees, and keep in a phial, and to shake the bottle whenever the remedy is used. In most cases it is sufficient to apply a few drops once a week to the parts affected. It will be seen that in three of the prescriptions for this complaint, given by Paulus Egineta, preparations of lead were used:—*℞*. 1. Of liquid alum, two parts; of myrrh, one part—dissolve in wine, and use. *℞*. 2. Plunge heated molybdena (oxide of lead) into fragrant wine, triturate with the wine, adding a little myrrh until it becomes of the thickness of the sordes in baths; then use. *℞*. 3. Of lithage, *dr*. xv.; of myrrh, *dr*. iii.; of ammom (cardamoms), *dr*. i.—mix with wine. *℞*. 4. Of liquid alum, *dr*. viii.; of ammom, of myrrh, and of spikenard—of each, *dr*. iv.; triturate with wine, and use. The commentator remarks: all the authorities concur in recommending for the cure of this complaint a combination of astringents and aromatics; they therefore direct us to mix alum with storax, myrrh, and the like (Paul. Eg., Book III. sect. xxxvi.). To render the perspiration fragrant was one of the toilet duties of the upper classes in Greece; it was generally done by the use of an ointment, in which were mixed the leaves of the cypress pounded dry, and the bark of the pine. That the breath also might be very agreeable, Paulus Egineta says:—‘One ought also to remember, in the morning, immediately after being dressed, to taste a small quantity of cassia or savin.’

ART. 82.—*On Syphilitic Acne.*

By Dr. TANTURRI.

(*Il Morgagni*; and *British Medical Journal*, May 19, 1866.)

Dr Tanturri recognises two forms of acne, the glandular and the follicular; and says that constitutional syphilis may manifest itself in either of them. In the *glandular syphilitic acne*, the inflammation attacks the epithelium, the proper wall of the sebaceous gland, and the neighbouring connective tissue. It differs from impetigo or eczema impetiginodes in the circumstance that, in the latter affection, the inflammation is limited to the epithelium of the gland, and yellowish friable, shining crusts are formed on the surface of the skin. These crusts contain a large amount of nucleated epithelium, like that which normally exists in the sebaceous glands, granular epithelial cells, pus cells, and

fatty matter. These elements are found in small quantity in syphilitic glandular acne; but the disease affects the gland and the surrounding dermis more deeply. It may be said that impetigo is characterized by a catarrhal inflammation, and syphilitic acne by a parenchymatous inflammation. Impetigo, Dr. Tanturri says, is not communicable by inoculation; but many experiments have shown that syphilis may be communicated to healthy subjects by the inoculation of the purulent contents of syphilitic acne. In glandular syphilitic acne, there may be recognised a stage of eruption or suppuration, one of retrogression or atrophy, and one of desquamation. Glandular syphilitic acne is generally one of the first constitutional manifestations of syphilis. Its development is slow and gradual; in some cases, however, it may be rapid—as it were acute, and is then accompanied with more or less fever. At the same time with the acute acne, we see erythema, moist papules, and other signs of recent constitutional infection. The chronic form is pretty frequently accompanied by ecthyma, gummata, chronic glandular swellings, periostitis, &c. Acute acne is one of the most obstinate symptoms of constitutional syphilis; the chronic is still more obstinate, especially if the skin have been affected by previous disease. Glandular acne, according to Zeissl, is never met with in newly-born syphilitic children; but Diday has described it without questioning its occurrence at this early age.

The diagnostic characters of syphilitic acne are thus described by Dr. Tanturri:—*a. Acute syphilitic acne.* In the eruptive period, the disease is seated on the trunk and the upper and lower limbs; the pustules are acuminate, with a hard base; the suppuration is central and superficial, and the pus has a tendency to dry up. In the suppurative stage, syphilitic acne presents pustules having a large hard base, with a central depression, covered by a small adherent crust. In its third stage, syphilitic acne is characterized by small hemispherical projections, desquamating at the circumference and having concrete pus in the centre. In the fourth stage, syphilitic acne presents small hemispherical projections with an irregular surface; superficial cicatrices, with little pigment; and an abundance of fine scales.

b. Chronic syphilitic acne is localized on the face, neck, trunk, and upper and lower limbs. It is characterized by small pustules with a slightly projecting base, which are developed slowly, and suppurate only at the centre.

Follicular acne presents the characters of a catarrhal inflammation limited to that part of the follicles which traverses the epidermis—that is, the portion lying above the opening of the sebaceous glands. The inflammation of that part of the follicle which is seated in the dermis or on the subcutaneous cellular tissue is, on the other hand, parenchymatous; because the wall of the follicle and the neighbouring connective tissue are equally affected, and participate alike in the formation of pus or of new connective tissue. When diffuse, follicular acne may, as well as the glandular form, be accompanied by more or less intense fever. It is generally an early symptom of constitutional syphilis; but Dr. Tanturri has seen it forming a kind of transition between the development of gummata and visceral lesions. The progress of follicular acne is slow, especially if left to itself; its duration is very long. If, at the

period of desquamation, the excessive development of epidermic cells continue for a long time, a condition resembling ichthyosis is produced. Follicular acne may coincide with all the tegumentary lesions of the first period of syphilis; and especially with iritis. The eruption may appear on any part of the body except the face; it consists of small pustules with a slightly projecting base, surrounded by a rose-coloured areola; the centre is yellowish, umbilicated, and traversed by a hair.

ART. 83.—*On the Treatment of Scabies by Oil of Petroleum.*

By Dr. DECAISNE.

(*Journal of Practical Medicine and Surgery*, January, 1866.)

From a report published in the *Archives Médicales Belges*, we learn that Dr. Decaisne has used the oil of petroleum successfully in upwards of six hundred cases of scabies. In the great majority of the subjects the disease was completely cured after a single friction, in several after two, and in very few instances were three or four inunctions required. The method failed in two or three cases only, and sulphuret of lime was necessary to effect a cure.

It has been objected that oil of petroleum is an irritant and produces rashes, but M. Decaisne remarks that the remedy applied with proper precautions seldom causes this unpleasant result.

“At first the frictions were performed with rough towels and brushes, and probably, in order to lacerate the sulci, the oil was rubbed violently into every part of the skin more particularly affected. The inevitable result was the exposure of the derm, and rashes consequent on the mechanical irritation. Military surgeons have, however, found from experience that this is unnecessary, and now the inunctions are more gently performed. But even this plan was open to improvement. It may be a matter of indifference when the skin is healthy to use a brush, a rough sponge, or a hard towel, but in the case of scabies the vesicles are often broken, and the cuticle destroyed, and the softest aquarelle brushes should be used to spread the oil on the integument.

“Since brushes of this description have been used in barracks the secondary eruptions have all but ceased, and when any have appeared they were the result of an error of diagnosis which cannot always be easily avoided in cases of some standing. Prurigo, eczema, impetigo, are often mistaken for scabies, and in these affections the evil effects of repeated and inopportune frictions are readily accounted for.”

M. Decaisne also adverts in his report to the disinfection of the clothing. Experiments instituted in the military hospital and the garrison of Antwerp have shown the utter inutility of the measures in habitual use. Since they have been discontinued, relapses have become less frequent, and the inutility of disinfection is, therefore, now fully demonstrated, and this expensive procedure, founded on routine and not on scientific experience, should henceforth be abandoned. If it be even

conceded for the sake of argument that the *acarus* can continue to live elsewhere than in its natural *habitat*, the operation would still be unnecessary, because in resuming his wearing apparel the patient exposes to the action of the petroleum with which his person is saturated, the few *sarcoptes* which may remain in his clothes.

The treatment with petroleum oil thus combines with its great efficacy the additional advantage of economy, because the process of disinfection is dispensed with, and the entire cost of the medication does not exceed for each case three or four centimes.

ART. 84.—*Lichen Ruber of Hebra.*

By THOMAS HILLIER, M.D. Lond., Physician to the Skin Department, University College Hospital ; Physician to the Hospital for Sick Children.

(*The Lancet*, July 21, 1866.)

The following case presents a good illustration of a rare form of disease, answering most nearly to what is called by Hebra *Lichen ruber*:—

E. J. G.—, a man aged sixty-five years, who has usually enjoyed good health, came under Dr. Hillier's care in University College Hospital in February, 1866. He stated that when about thirty years of age he had a rough scaly patch on the outside of each of his forearms, which remained about a year and then disappeared. He had usually, when well, a rather harsh skin.

About six weeks before admission he first noticed that his hands were stiff and difficult to open and shut ; there were also cracks at the bottom of the flexures. About the same time he found that the limbs and trunk became rougher, and in many parts covered with fine white scales ; his feet also had become stiff, very dry, and cracked, and the nails of the fingers and toes were much thicker than formerly.

Soon after admission the following notes of his condition were taken :

“ Patient complains of nothing except the stiffness of his hands and feet, with soreness from the presence of deep cracks. He can with the greatest difficulty make any use of his hands, from the impossibility of closing the fingers upon the palms. There is but little itching. His pulse is of moderate strength, 84 in the minute ; his tongue is coated with a white fur, cracked near the middle, and he has a clamminess and sour taste in his mouth ; his bowels are regular ; his limbs are rather thin.

“*Description of the skin.*—Nearly the whole of his trunk, back and front, was covered with a thin layer of white scales, in places separable without difficulty in fragments about the size of a finger-nail, or smaller ; the layer of scales was nowhere thicker than stout writing-paper. There is a large collection of scales between the sheets every morning when his bed is made. After taking two warm baths, many of the scales were detached, and the whole front of his trunk, except near the epi-

gastrium, was red and dry, the ridges and furrows of the skin being a little exaggerated. Here and there are small patches of skin of the natural colour; around the margins of these are numerous papules, the size of pins' heads, each surmounted with a fragment of cuticle. It is quite evident that all the scales are produced by the desquamation of such papules, closely aggregated until they coalesce. In many cases a small sheath of cuticle is seen around a fine hair as it emerges from the follicle. In the right groin there are confluent papules on a reddened surface. In the left groin there is natural skin, dotted with brownish-white desquamating papules. Thighs:—On the front is seen a layer of scales, reminding one of slight ichthyosis. This extends over the right knee, and partly over the left; the skin of the legs is in part scaly and in part reddened with prominent hair follicles, each surrounded with a small fringe of cuticle. Feet:—The skin is very hard, thickened, and in places very deeply cracked. The toe-nails are four or five times their usual thickness at the free extremity. It is impossible to cut them with an ordinary pair of scissors. The skin of the arms and forearms is like that of the trunk; below the middle of the forearm are seen a great many hairs, broken off short, surrounded by a white fragment of cuticle. The hands are dry, stiff, hard, and deeply cracked, especially on the knuckles, and to a less degree on the palms. The finger-nails are much thickened, and for some distance back from the free extremity are opaque. The cuticle of the hand is separable near the deep cracks in thick fragments of some size; elsewhere it is adherent to the cutis, as in health. Face free from eruption, except the whiskers and beard, where there is fine desquamation around each hair, as is often seen in *tinea tonsurans*. Some *ptyriasis* of the eyebrows. External ears harsh, covered with dry scales, especially on the outer aspect and near the meatus. Scalp, on admission, swarmed with pediculi; it is now shaved, and presents an appearance as if covered with a layer of flour and soap allowed to dry on. On closer examination there are seen abundant fine scales between, and in many cases sheathing, the hairs. He has an unusual crop of hair for his age. The hairs do not appear to be brittle or loosened in the follicles. Under the microscope no change is seen, except that a few of them exhibit a fibrous fracture. The scales of cuticle are found to be infiltrated by numerous globular bodies, strongly refracting the light, not soluble in ether or liquor potassæ. They look like vegetable spores, but there is no appearance of filaments of mycelium."

Bichloride of mercury and arsenic disagreed with this patient, but by maceration of the skin with water-dressing, inunction of oil, and the use of citrate of potash, for two months, he improved greatly.

(G) CONCERNING THE MUSCULAR SYSTEM.

ART. 85.—*On Hypertrophy of Muscle.*

By EMIL STOFFELLA and Prof. W. GRIESINGER.

(Wien. Zeitschr. 1865 ; Arch. d. Heilk. 1865 ; Schmidt's Jahrbücher, 1865.)

Dr. Duchenne, in his classical treatise upon localized electrization, has made mention of the occasional occurrence of hypertrophy of muscle in connexion with brain disease, and observations of the same kind have since been published by Schützenberger, Spielmann, Jacksch-Kaulich, and Berend. The two most recent writers, named at the heading of this article, have examined into the matter very thoroughly; and Griesinger especially has explained the pathological anatomy of the process.

The case recorded by Stoffella was observed in the clinic of Oppolzer. The disease developed itself quickly, although gradually, in a previously healthy boy of thirteen, who had just recovered from measles, following soon after variola.

At first there appeared only symptoms of weakness of the legs, with which was afterwards associated an increase in the circumference of the muscles, limited almost entirely to the calves, the extensors of the leg, the deltoid and triceps, the sacro-lumbalis, and longissimus dorsi. The hypertrophied muscles were paretic and harder to the touch than natural. Their electric contractility was destroyed, but in the muscles not hypertrophied, with a few exceptions, the electro-muscular sensibility was normal, although somewhat increased in the extremities. The nervous reaction was natural, but in some parts too strong, in response to currents through the spinal cord, and from nervo-muscular currents there was slight diminution of reaction. During seven months' treatment, conducted by Benedikt, the irritability gradually decreased, but the other conditions remained unchanged.

Dr. Stoffella considered the malady to be cerebral, with no determinate seat. The want of other brain symptoms was no evidence to the contrary, since these are often absent; nor the loss of electric contractility, which is common in affections of the base of the brain. This loss, therefore, might be taken to indicate that the base of the brain was the seat of the disease, while the fact that the extensor muscles were chiefly affected rendered a central cerebral or spinal affection probable. That the brain was the organ primarily affected appeared likely from the frequent occurrence of muscular hypertrophy in idiocy and paralytic amentia, hypertrophy, especially of the lower extremities, also from the absence of disorder of the bladder or rectum; and lastly, from the general resemblance of the case to Duchenne's description of cerebral paraplegia, in which hypertrophy of the lower extremities commonly occurs, although the electric contractility remains normal.

Professor Griesinger's patient was also a boy thirteen years old, who

had suffered since earliest childhood from weakness and slight thickening of the legs.

In his tenth year his condition became much worse than before, the paralysis and hypertrophy increased considerably, and the thighs were attacked from time to time by momentary cramps. In his thirteenth year he was no longer able to walk; and when in bed could not move the legs without great difficulty. The muscles most hypertrophied were those most completely paralysed—namely, the extensors of the leg, and the flexors of the leg and foot. In the upper extremities also there existed a high degree of paralysis of individual muscles; and of these also the most enlarged were the most enfeebled. They were the deltoids, and a portion of the muscles of the scapula. The forearms seemed to retain their normal condition; the arms participated in the morbid change in some degree. On the trunk the quadrati lumborum and recti abdominis were chiefly affected; the muscles of the head and neck were perfectly sound. The affected muscles became hard and swollen upon any attempt to use them. They acted to the induction current, but only feebly; and the apparently sound muscles exhibited a diminished reaction. The cutaneous sensibility was generally normal. Upon the lower extremities, and also upon the weakest parts of the upper, there often appeared, especially upon attempts at voluntary movement, a paler or darker rosy flush, accompanied with elevation of temperature; and succeeded, if the part were long exposed, by a bluish marbling and slight coldness. The senses were normal, mental faculties perhaps a little dull, micturition normal, bowels somewhat costive.

As the general character of the symptoms appeared to point to degeneration of the hypertrophied muscles, Griesinger determined to satisfy himself upon the point by the excision of a portion of one of them; and he accordingly cut out a piece of the almost functionless deltoid. This was of a pale yellowish-white colour, and was so loaded with fat between the muscular fibres as to resemble common adipose tissue. This was especially true of a transverse section, which differed from adipose tissue apparently only in containing a more abundant connecting membrane.

Griesinger hence concluded that the disease was essentially a myopathic paralysis, that the disease of muscle was largely diffused over the body, that the hypertrophy was only one, although probably the final, phase of its progress, and that the apparently normal, or even atrophied muscles, were really in earlier stages of the same affection.

A treatment of the affected muscles by compression was pursued for four weeks without benefit; but gymnastic exercises, with iodide of potassium, and a diet calculated to arrest fat formation, acted favourably.

At the conclusion of his paper, Griesinger gives a brief account of the cases previously observed, and finishes with the following general statement:—

“There exists, as a rare disease of childhood, an increase of volume of voluntary muscles with diminished contractility. It appears to depend upon a destruction of the primitive muscular fibre by an abundant development of fat. It is most common in the male sex. It may

probably be congenital in its origin, but undergoes more rapid development in the second period of childhood, when it may sometimes be said to become acute. It affects chiefly the legs, thighs, and, in the arm, the deltoid. The remaining muscles are often thin and lax, and possessed of little contractility."

ART. 86.—*On Granular Degeneration of the Voluntary Muscles.*

By Dr. L. MERYON, F.R.C.P.

(*The Lancet*, March 10, 1866.)

The following is an abstract of a paper read before the Royal Medical and Chirurgical Society :—

Since 1851, when Dr. Meryon communicated to the Society an account of this disease, the details of which were published in its *Transactions* in 1852, four more cases have fallen under his observation. In one, which terminated fatally in 1859, the post-mortem examination was carefully conducted by Mr. Savory, who, during a searching microscopical investigation, failed to detect a vestige of disease in any part of the nervous system. The disease appears to consist in a gradual but progressive breaking up of the amorphous membrane which envelopes the primitive muscular fibres, and of a dispersion of the contained granular matter. After these preliminary observations, Dr. Meryon proceeded to describe the case of a gentleman, aged twenty-two, who was present at the meeting, so that the fellows had an opportunity of examining his present condition. At the age of five years he began to show symptoms of weakness in the loins by a waddling gait; and, in the course of two or three years, he had difficulty in bending the thighs on the body, as in the act of getting up stairs; next he gradually lost the power of bending the legs on the thighs; and eventually the voluntary motions of the feet and toes were lost also. The morbid action then began to manifest itself in the upper extremities, and the patient has now no longer the power of raising his arms to his head; but he can bend the forearms on the arms, and he still retains a firm grasp with the hands. The tendency of the disease, however, is to extend itself from the proximal to the distal portions of the extremities, and then to attack the muscles of respiration; but in no case are the involuntary muscles or muscles of organic life affected. The history of one case is the history of all; and on the uniformity of the symptoms and the order of their appearance, &c., Dr. Meryon has attempted to distinguish this form of paralysis from all others which are dependent on lesions of the peripheral nerves or nervous centres—1st, by the centrifugal course of the disease, irrespective of the course and distribution of nerves; 2nd, by the entire absence of any symptoms of nervous disturbance during life; and 3rd, by the absence of any trace of lesion in any part of the nervous system after death—at least so far as the most

careful investigations have extended up to the present time. There is another difference observed in the muscles thus affected, as compared with paralysed muscles dependent on nervous lesion: it is in the disruption of the sarcolemma and the segregation of the granules which constitute the sarcous matter in the former case; whereas, in the latter, the primitive fibres gradually waste, the transverse striæ gradually disappear, and oil-globules by degrees fill the interspaces of the fibres, and occupy the space which the healthy muscular tissue formerly occupied. Dr. Meryon's patient affirms that both he and his sister, who is also a subject of the disease, have retained the condition in which they were when they began to take arsenic. The gentleman has been under the influence of that medicine a little more than a year. In conclusion, Dr. Meryon adverted to the question of priority of description of this peculiar form of disease. He quoted passages from several of the French medical periodicals in relation to this matter. It is unnecessary to report these, though the following fact may be mentioned—that at the Academy of Medicine M. Cruveilhier referred to Dr. Meryon's plates in illustration of his (M. Cruveilhier's) own diseased muscles, and called the attention of the Academy to a form of paralysis "*non encore décrite*." Dr. Meryon's paper had been published in *The Lancet* more than a year before, and shortly afterwards appeared also in the Society's *Transactions*.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

ART. 87.—*A New Method of Treatment, by which Malignant Tumours may be Removed with little Pain or Constitutional Disturbance.*

By W. H. BROADBENT, M.D.

(*Medical Times and Gazette*, September 1, 1866.)

The attention of Dr. Broadbent was directed to the treatment of cancer under the following circumstances:—In 1864 he was consulted by a lady suffering from cancer of the breast. By his advice, the breast was removed by Mr. Walter Coulson. The disease returned, and was again removed in August, 1865. In May of the present year, a tumour was growing more rapidly than ever near the cicatrices of the former operations. It was decided that no further removal was advisable; and, unless something could be done, a miserable fate was before the patient. The hypodermic syringe is now in the hands of every physician; and it seemed to the author that by it some fluid might be injected into the tumour which might so far alter its structure and modify its nutrition that its growth might be retarded or arrested. After considering the various substances which presented themselves to his notice, he selected acetic acid for the following reasons:—1. This acid does not coagulate albumen, and might, therefore, be expected to diffuse itself through the tumour; and the effects would not be localized at the point injected. 2. If it entered the circulation, it could do no harm in any way. 3. Acetic acid rapidly dissolves the walls and modifies the nuclei of cells on the microscopic slide, and might be expected to do this when the cells were *in situ*. 4. It had been applied with advantage to common ulcerations. On May 18 the first injection was practised. The tumour was about the size of a small egg, and a patch of skin of about the size of a shilling had become adherent to it. The needle was introduced through sound skin an inch or more from the part involved in the disease, and passed to the centre of the mass. About thirty minims of dilute acid (one part of acid to one and a half or two of water) were injected. It gave little or no pain. Next morning a bulla containing dark bloody fluid was found to occupy the patch of adherent skin. May 23.—This portion of skin dry, hard, and horny; the adjacent part of the tumour not so hard. Again injected. The patient, residing in the country, was not again seen till June 7, when the piece of skin

mentioned was found detached from the surrounding sound skin; and a probe could be passed in all directions to a distance of three-quarters of an inch or more between the tumour and the healthy structures. A little discharge issued from the fissure mentioned. Injected on this date, and again on the 9th, the acid used being rather stronger. It gave a little pain, and swelling and tension of the parts around followed. On June 13, and a few days afterwards, there was a free discharge of fluid and solid portions, with relief of the swelling, &c. No fœtor whatever attended this discharge, which afterwards diminished greatly. Seen again on June 26, when, on external examination, the tumour was found to be much smaller; and, on passing a probe into the opening, it entered a large cavity extending on all sides. Part of the walls seemed free from malignant structure, but at several points a crust of cancerous deposit remained. On attempting to inject, it was found too thin to retain the fluid, which either entered the tissues and gave great pain, or made its way into the cavity. The cavity was stuffed with lint saturated with dilute acid, and the case left in the care of the family medical attendant, who was to inject as he saw opportunity. July 13.—No impression made on the remaining disease, which had, in the opinion of the medical man, extended somewhat. Carbolic acid was tried for a few days as an application, but discontinued, and the cavity dressed daily with strong acetic acid by the medical attendant, and injections practised daily. This energetic treatment gave much pain, and excited inflammation all round. When again seen by the author on August 4, there had been considerable hæmorrhage, which had been arrested by free application of tincture of sesquichloride of iron. The result, however, was apparently the entire removal of the remains of malignant disease; and, when last seen, a healthy granulating surface was left at every point. Three other cases were related by the author. The author further formulated certain conclusions from the experiments detailed, and stated the cases to which, in his opinion, the treatment was not applicable. Guided by his experience, he considered large quantities of dilute acid preferable to stronger acid; and he would not, without great hesitation, attempt the destruction of any tumour which had not involved the skin. His aim had originally been, as stated in the early part of the paper, not necrosis of malignant tumours, but a modification in their nutrition. The theoretical grounds for this hope were, that cancer owed its malignancy to its cellular (to use a nomenclature now almost antiquated) or fœtal structure; and that in acetic acid we had an agent which might be expected to diffuse itself through the tumour and reach the cells, and, having reached them, to effect changes in their structure, and affect them vitally, while it could scarcely do harm. The results he had brought before the profession at the earliest possible moment. The ultimate value of the treatment he left to be decided by a more extended experience. It was important to use large quantities of dilute acid, and not to have the acid too strong.

ART. 88.—*On the Employment of Galvanism in promoting the Cicatrization of Sluggish Sores.*

By Mr. NUNN, Surgeon to the Middlesex Hospital.

(*The Lancet*, July 28, 1836.)

Mr. Nunn records the following cases. In treating ulcerations he finds it indifferent whether an induced or continuous current be used. He has often used Pulvermacher's galvanic chain with advantage in obstinate sinuses.

CASE 1.—W. F——, aged twenty-three, had benign fungus of the testis, the sequence of strumous abscess. There was a circular perforation of the scrotum on the left side as large as a florin, and it had existed during twelve months. The fungus was about the size of a walnut; there was a free discharge from it of yellowish glairy purulent fluid.

Oct. 12th.—Galvanism of weak intensity (from a single cell), to be applied for five minutes every morning. The effect of the galvanism was most remarkable: the fungus receded, and the edges of the perforation contracted over it, after a few days.

27th.—A slightly stimulating lotion of nitrate of silver was ordered (half a grain in the ounce of water).

30th.—The solid nitrate of silver was brushed round inside the sore, which had now nearly closed; and nitric acid and decoction of bark were ordered to be taken three times daily.

Nov. 6th.—The patient was discharged, the sore being quite healed.

CASE 2.—H. D——, aged five years, was admitted with an unhealed sinus, probably caused by caries of one of the cuneiform bones. The swelling was chiefly on the inner side of the foot, in front of the instep. The tarsal end of the metatarsal bone of the great toe was enlarged. There was the history of a sprain; and the child's parent believed the disease of the foot to be due to the sprain. An incision had been previously made in the part, and some unhealthy bone scooped away by another surgeon. The child had a scrofulous appearance, and its belly was tumid. Mr. Nunn ordered nitric ether in half-drachm doses, with syrup, three times a day—a medicine which he believes to exert the most useful influence in promoting the nutrition of cachectic children, and to have the power of diminishing that distension of the abdomen so characteristic of deficient nutrition from sluggishness of the mesenteric glandular system. Iodide of potassium ointment was also prescribed to be rubbed into the belly. These measures were carried out from Jan. 27th to Feb. 11th, without much change in the appearance of the sinus, when galvanism was ordered to be applied for five minutes three times a week. The effect of the galvanism was at first to diminish the angry redness of the part, and also the amount of discharge from the sinus. By the end of the month the sinus had scabbed over, and the patient was discharged on the 7th of April convalescent.

CASE 3.—Jane D——, aged twenty-two, admitted Dec. 12th with angular curvature of the spine. She was healthy up to the age of fourteen years, when she began to suffer from rheumatic pains "all over her." There was at the date of admission an opening in the upper third of the right thigh, about the apex of Scarpa's triangle. Four years since there was some swelling and stiffness of the thigh; two years since, at the seat of the opening, a swelling as large as an egg. The spine projected in the lumbar

region : there was some tenderness on either side of the spinous processes ; and occasionally at night, and at change of weather, pain in the part was severe.

Iodide of potassium, in three-grain doses three times a day, was ordered ; and directions were given to double the dose in the event of increase of pain at night. Iodine paint to the back.

On the 26th, galvanism was directed to be applied twice a week, the current being passed from the thigh to the spine. This was continued until February 18th, when the patient was discharged much improved. The sinus in the thigh had nearly closed.

CASE 4.—Agnes D—, aged four, admitted Nov. 14th, having a superficial strumous ulcer, with somewhat excavated edges, surrounded by small pustular elevations, on the dorsum of the left hand. There were similar sores on the foot and face. Iodide of potassium in one-grain doses twice a day, with cod-liver oil, were prescribed.

On the 17th, galvanism for five minutes was ordered to the foot, twice a week ; on the 28th, to the hand and face also. This was followed almost immediately by a change to a healthy action in the sores. The patient was discharged convalescent January 9th.

ART. 89.—*On the Treatment of Wounds by Ventilation.*

By M. BÉRENGER FÉRAUD.

(*Bulletin de Thérapeutique*, Jan. 31, Feb. 15, 1865 ; *British and Foreign Medico-Chirurgical Review*, July, 1866.)

This method of treating wounds consists in leaving small wounds exposed to the air, and in acting upon larger ones by means of the domestic bellows for a period varying from five to twenty minutes every two, three, or four hours, according to the amount of discharge and moisture that may be present. The object is to secure the formation of a crust over the surface of the wound, under which cicatrization takes place far more rapidly than when the surface is not so protected ; and the applications must be sufficiently frequent and prolonged to maintain this crust of a certain thickness. When the crust acquires a degree of rigidity, however, it must be displaced and another formed ; and when the discharge is very abundant, the alcoholic dressings, now so much in vogue in the Paris hospitals, should for a while precede the ventilation. The influence of this last in improving the condition of the wounds is almost immediate, a disposition to cicatrize and a diminution of the discharge soon being apparent.

This mode of treatment, according to its originator, M. Bouisson, of Montpellier, may determine sedative, astringent, siccative, antiseptic, and tonic action ; but it is by no means indicated in all kinds of wounds, and especially in those whose depth is great in proportion to their superficial extent. Thus, it is not fitted for penetrating wounds, as those of a fistulous character, or characterized by anfractuosités. Abundant suppuration is a further contra-indication, except, indeed, when this is due to a mere hyper-secretion dependent upon local or general atony or perverted nutrition, and to the lessening of which

alcoholic dressings supply a useful preliminary to the employment of ventilation. In slight burns other means may be preferable, as of more convenient application; but in those of the second and third degree, arrived at the stage of a simple denuded wound, ventilation may advantageously supersede cotton and other impermeable applications. In resorting to this means for ulcers, we have to attend to the constitutional cause of these, as well as to render them by various local applications apt for cicatrization before we resort to ventilation.

Among the secondary advantages of this mode of treatment may be mentioned its simplicity, its easy applicability by the patient or his friends, its economy and its cleanliness. It substitutes a dry for a moist surface, diminishes the chances of septic decomposition, and lessens the chances of infection of the surrounding atmosphere.

ART. 90.—*Use of the Acupuncture Needle in the discovery of a Pistol Ball.*

By Dr. GORDON BUCK.

(*New York Medical Record*, June 15, 1866; *American Journal of Medical Sciences*, July, 1866.)

At a meeting of the New York Pathological Society, Dr. Buck stated that he had successfully employed the acupuncture needle in detecting the ball in a case of gunshot wound in a man who, while carrying a pistol in his fob, the weapon was accidentally discharged and its contents were lodged into the groin immediately below Poupart's ligament, just two inches outside of the femoral. On introducing a probe into the wound of entrance, it followed a track over the inner condyle and a little above it, and at its bottom a firm body was encountered that was about the size and shape of the missile that was supposed to have been lodged there. This body could be slipped within a certain limit, and its movement would cause pain. Presuming that it was the ball, there was not certainty enough in the diagnosis to warrant an attempt at its extraction until the acupuncture needle was used. This was passed down in the situation of the deep-seated lump through the tissues, and encountered the foreign body. By certain manipulations it was found to escape from the point of the instrument and roll aside, which fact left no doubt in the mind of the presence of the projectile at that point. It was then cut down upon with a narrow-bladed knife, and removed without difficulty. Dr. Buck remarked that his attention had been first called to the needle by seeing a published account in some of the medical periodicals of its use by a Scotch army surgeon, whose name he did not recollect. Dr. Buck also stated in this connexion that he had employed the same procedure with success in discovering the existence of a calcareous body impacted in the prostate gland. The needle in this instance was curved, and was introduced into the gland upon the finger as a guide. The needle is very fine, and has a trocar point in order to facilitate its entrance into the tissues.

ART. 91.—*The Results Attending the Removal of the First Growth of Cancer.*

By Mr. JOHN BIRKETT, Surgeon to Guy's Hospital.

(*British Medical Journal*, September 20, 1866.)

What advantage does a patient obtain in submitting to the removal of a cancerous tumour? The answer which Mr. Birkett gives to this question is too full of interest to be curtailed. He writes:—

“The facts upon which to base a reply to this inquiry are derived from the investigation of a hundred and fifty cases carefully recorded by myself; and, although I have not always performed the operation, I have seen the patient and examined the growth after its removal. A majority of the patients are dead; for it should be borne in mind that this collection of cases was commenced eighteen years since, and that not a little difficulty arises in being able to follow out patients who survive several years.

“Also, it must be stated that I have not made any selection of the cases with the view to uphold or support any particular statement. The sufferers who succumbed to the disease were placed in the order in which death occurred, and therefore some allowance should be made in those cases in which death ensued very rapidly after the development of the disease appearing on the tables in greater numbers than those which survived the same thing many years.

“The above consideration, as well as others, render what are termed statistical tables, and deductions therefrom by means of averages, most fallacious guides to treatment.

“Table A is arranged to show the length of time during which 150 patients were free from any indications of the local recurrence of the disease after the removal of the first growth.

A.—*Table showing the Length of Time during which 150 Patients were Free from Indications of the Local Recurrence of the Disease after operation.*

	Cases.
Before the expiration of the first year	87
Between expiration of the first year and close of second .	15
“ second and third	7
“ third and fourth	7
“ fifth and sixth	5
“ sixth and seventh	2
“ seventh and eighth	1
“ eighth and ninth	3
“ ninth and tenth	1
“ tenth and eleventh	3
“ fourteenth and fifteenth	1
“ fifteenth and sixteenth	1
Sixteen years	2
Patients died free from local disease in part first affected (see table)	15

"Before the expiration of the first year, eighty-seven patients showed signs of a new development of the growth, either in the portion of the mammary gland not removed, the integuments in the neighbourhood of the cicatrix, in that structure itself, or in the axillary lymphatic glands.

"After the expiration of the first year, and before the conclusion of the second, fifteen patients showed that a cancerous growth was again developed in one or other of the regions above mentioned.

"Now, this large proportion of the cases in which recurrence occurred, might be taken as a significant fact to demonstrate that the cases submitted to operations were badly selected; that, indeed, an operation was scarcely justifiable. But, in many of the cases, the operation was undertaken in the hope of removing a source of great local pain and mental distress; of alleviating the misery and to abate the annoyance attending an ulcerated and sloughing surface, and at the earnest solicitation of the sufferer.

"In some, I confess, little, if any, advantages were gained. In others, although life was not prolonged by many months, the existence of the individual was rendered more tolerable, since the attendant circumstances before described were sometimes absent. Life was decidedly prolonged in a few cases, in which it was rapidly ebbing in consequence of repeated hæmorrhages and deeply sloughing masses.

"Further, we may be allowed to suggest that many of the cases in this category might have been operated upon at a much earlier period after the discovery of the first growth, and with every probability of a happier result. But, in hospital cases, and a large majority were of that class, it too often happens that patients apply to such institutions as a last resource only.

"We may now turn to a somewhat brighter picture. To be free from such a disease as cancer for periods of time extending between three and sixteen years, is a fact surely sufficient to justify almost any means to accomplish such a desirable end. The risk to life attending the operation is not great, and now much of the horror of such a proceeding is mitigated by the employment of anæsthetics.

"In the wards of a hospital, even where the chances against the recovery of the patient are greater than in private practice, I calculate the death-rate at only seven per cent. During the last seventeen years, two hundred patients have been operated upon by my colleagues and myself in Guy's Hospital. Either the whole or a portion of the breast-gland was removed on account of a carcinomatous growth. All of these recovered from the effects of the operation, with the exception of fourteen, who survived it between three and thirty-six days only. It must be admitted that the operation was more or less the exciting cause of the disease which terminated life. These fatal diseases were erysipelas, followed by bronchitis; inflammation of the pleura, terminating in effusion; pyæmia; hæmoptysis; and vomiting. In fact, the too common causes of fatal complications after operations upon the poorer classes, inhabitants of large cities.

"But in private cases the mortality is so trifling that, admitting the risk to which every person submits who undergoes any operation, I am

inclined to calculate it at not more than three or four per cent. I have lost only one patient, of forty-one cases operated upon for cancer.

"To proceed with the remaining cases. Of the patients, thirty-three in number, who survived the operation without any local recurrence of the cancer for periods varying between two and sixteen years, assuredly many of them must have died of the complaint within those periods; and all of them would certainly have been compelled to endure the mental anguish, if not the local suffering, accompanying the existence of this terrible malady, assuming that they had survived equal periods.

"Lastly, fifteen of the patients died without showing external signs of recurrence of cancer in the region first affected.

B.—Cases in which the Cancer did not Re-appear in the part first affected with that growth.

Cases.	Survived operation.	Cause of death.	Condition of local disease at operation.
1	6 months	Hepatic disease	Integuments infiltrated
2	10 months	Thoracic disease	In same condition
3	11 months	Hepatic disease	As above
4	13 months	Carcinoma in calvaria	Mammary gland only infiltrated
5	15 months	Disease of ovary	As above
6	2 years	Cerebral disease	Integuments infiltrated
7	2 yrs. and 2 months	Hepatic disease	Integuments infiltrated & ulcerated
8	3 years	Thoracic disease	Mammary gland infiltrated only
9	4 yrs. and 3 months	Cerebral disease	Same as above
10	6 years	Thoracic disease	Integuments infiltrated
11	6 years	Cerebral disease	Integuments infiltrated & ulcerated
12	6 yrs. and 6 months	Exhaustion	Mammary gland infiltrated
13	6 yrs. and 8 months	Thoracic disease	Integuments infiltrated
14	10 yrs. and 6 months	Cachexia	Integuments ulcerated
15	11 years	Cachexia	Mammary gland infiltrated

"The Table B shows the length of time each individual survived the operation. This was between six months, the shortest time, and eleven years, the longest. In another column is stated the cause of death in each case, which was the development of cancerous growths in the viscera of either the cranium, the thorax, or abdomen, as determined by well-marked indications during life or by *post-mortem* examinations.

"I have introduced, in the same table, as brief a description as possible of the condition of the local disease at the time of the operation; and it should be noted that it had made considerable progress in some of them. The integuments were infiltrated with cancer; in some ulceration of the surface existed. Under these conditions, we are justified in assuming that some of the patients would speedily have fallen victims to the ravages of the complaint, and that all must have endured more or less of the suffering accompanying its progressive stages.

"By the removal of the growth, these fifteen patients were exempt from the misery inseparable from the activity of the local disease.

"Let us next inquire if the life of individuals afflicted with cancer of the breast is prolonged by the removal of the part first involved by the disease.

c.—Table to show the Number of Years 150 Patients survived the Discovery of the Disease after the Removal of the First Growth.

Under 1 year	8	Above 10 years	2
Above 1 "	24	" 11 "	2
" 2 "	38	" 12 "	1
" 3 "	17	" 13 "	1
" 4 "	21	" 14 "	2
" 5 "	7	" 15 "	1
" 6 "	5	About 23 "	1
" 7 "	10	" 29 "	1
" 8 "	4	" 32 "	1
" 9 "	4		

"I have arranged Table c to show the number of years one hundred and fifty patients survived the discovery of the disease after the removal of the first growth. Rather more than one-half died before the expiration of the fourth year, or in the ratio of fifty-eight per cent.; the majority dying before the completion of the third year.

"Thirty-three died before the expiration of the seventh year, or in the ratio of twenty-two per cent.

"Eighteen died before the conclusion of the tenth year, or in the ratio of twelve per cent.

"Twelve survived about ten years, or in the ratio of eight per cent. One person lived about thirty years after the discovery of the disease.

"In order to form some comparison between cases subjected to the above treatment, and those in which the disease was allowed to pursue its natural course, with the exception of using local palliatives and constitutional measures, I calculated the average duration of life of a hundred patients.

"Fourteen of these patients died within the first year after the observation of the disease; three survived its discovery above ten years, two of them having lingered under its slow progress about twenty-six years.

"The average duration of life I believe to be about three and a half years.

"Of the cases, then, which have fallen under my own observation, it is quite certain that the longest survivors have been those from whom the first growth was removed.

"Whether the duration of life was really essentially due to the removal of the first growth, I would not venture to assert dogmatically; for there are many collateral circumstances which require to be taken into consideration, for which the time is insufficient upon the present occasion.

"In conclusion, I trust that I have demonstrated to my sceptical professional brethren that a certain proportion of cancer patients can receive benefit by submitting to the removal of the first growth of the disease; and that the benefit derived from the operation is two-fold—viz., 1, prolongation of life; 2, exemption from the disease for a considerable period of time in many instances."

ART. 92.—*Treatment of Large Abscesses by Drainage.*

By Dr. O'FERRALL, Surgeon to St. Vincent's Hospital, Dublin.

(*Medical Press and Circular*, June 6, 1866.)

R. R.—, aged nineteen, by occupation a labourer, residing at Chapel-izod, was admitted to St. Vincent's Hospital on the 17th of April, 1866, under the care of Dr. O'Ferrall. It appeared that on the previous March 17th, he fell on his hip, and at once went to a hospital where he was treated for the injury. Some days previous to leaving hospital he complained of severe pain in the left shoulder, to which was applied a liniment, and he was discharged. Three days after he left the hospital referred to he sought admission into St. Vincent's Hospital. At the time of his admission he was observed to have a large tumour situated on the left scapula extending backwards beyond the vertebral border, and engaging both supra- and infra-spinous fossæ; the prominence of the spine being completely obliterated, and extending below and without as far as the lower margin of the latissimus dorsi. There was great increase in the breadth of the shoulder with an œdematous hard swelling occupying the outer portion of the supra-clavicular fossa. At the time of Dr. O'Ferrall's first visit (22nd April) it could be felt to fluctuate, appeared that the patient had rigors twice since his admission, and its and experienced pain of a more or less acute character.

On the 26th April Dr. O'Ferrall inserted a drainage tube at about the level of the spine of the scapula, carrying it downwards to a little below the angle, when it emerged, and through it a considerable quantity of healthy pus was discharged, with great relief to the patient. The discharge gradually diminished.

The patient was treated with wine and bark, and when Dr. O'Ferrall I last saw him on the 14th of May, the tube had been withdrawn, and the tumour had completely disappeared. At the same time it was noticed that the man's general health was much improved by his stay in the hospital. At no period was there an offensive odour from the pus. There was a slight movement of the tube made once each day to prevent the aperture becoming clogged, which necessarily gave him great pain when withdrawing the tube. Dr O'Ferrall made use of a simple manœuvre—viz., firmly fixing the elastic tube between the forefinger and thumb of one hand, while withdrawing with the other hand the loose end; by this means the length of the tube is increased, while its calibre is diminished so as to enable the surgeon to draw it through its track without exciting the slightest pain; and if desirous of removing the tube, by suddenly letting go the end held between the finger and thumb, by its elasticity it passes with a sharp click through the track without giving the least pain.

Referring to the practice of drainage in large abscesses, Dr. O'Ferrall remarked that two facts of practical importance were illustrated by this case.

1st. That during the progress of treatment no offensive odour of any kind in the matter flowing through the tube could be detected. This

he attributed to the perfect ventilation by the perforated tube and the constant escape of the pus as rapidly as it was formed, and before it would have time to undergo any change by decomposition.

2nd. The employment of a simple mode of withdrawing the tube without causing the pain which the least movement in the daily dressing of the part had previously occasioned. The tube is held at both extremities, and extended by traction so as to diminish its calibre to the smallest possible amount. One extremity is then suddenly let loose, and the tube is found to have escaped from the wound with a sharp click and without the production of pain.

SECT. II.—SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 93.—*On the Sub-conjunctival Injection of a Solution of Chloride of Sodium to promote the Absorption of Corneal Exudations.*

By Professor ROTHMUND.

(*Mon. Bl. für Augen-heilk.*, 1866 ; *Schmidt's Jahrbücher*, 1866.)

It is well known that a great variety of means have been used to promote the absorption of corneal opacities left behind by parenchymatous keratitis ; and that the most successful of these have excited some degree of inflammation. Prof. Rothmund has lately used for this purpose a solution of common salt in distilled water (3j. to 3j.). A small quantity of this solution, previously warmed, has been injected under the conjunctiva, by a syringe with a slightly curved point, introduced about a line and a half, or two lines, from the margin of the cornea. The injection is made very slowly, and produces a *quasi* chemotic swelling around the cornea. This swelling is absorbed in about five or six hours, under a compressive bandage. The inflammatory symptoms produced subside in a few days ; and the cornea begins to clear from the periphery towards the centre. After three or four weeks the injection may be repeated ; and after from three to five injections it is usually possible to make an artificial pupil. Further experience of the value of this method is required.

ART. 94.—*On Détachment of the Retina : its Causes and Treatment.*

By Mr. HAYNES WALTON.

(*British Medical Journal*, June 16, 1866.)

In a paper read before the Harveian Society, Mr. Walton observed that detachment of the retina from the choroid might be the result of

an accident, such as a blow on the eye-ball or about the orbit; but for the most part, it could not be traced to an injury. It was the physical effect of fluid effusions of various natures, chiefly, however, serous, or of firm solid deposits, or of malignant disease. It was the class of cases produced only by the pouring out of serum, "dropsy under the retina," that he should consider. This separation of the ocular tunics was of common occurrence, and was one of the greatest mechanical changes that occurred in the eye, and yet was one without any external or objective symptoms. It was only by an ophthalmoscopic examination that the true nature of the case could be made out. There were undulating folds of the retina, or bulging of the retina in a tense form, the colour of the membrane varying from a light blueish or greyish tint, to a dead white, which was characteristic of old detachment. The recognition of the retinal vessels removed doubt of the diagnosis. The detachment might be partial, destroying sight to a limited degree, or general, totally annihilating vision. The tendency in the affection was always to become worse, so that a worse class of cases could not occur. There was no opportunity for the natural reparative power. It was Mr. Walton's conviction, however, that there was scope for treatment and opportunity for success; and that this consisted in general and local measures; but the treatment should be commenced early, or little benefit could be expected. He deprecated the idea that a mere operative proceeding could be curative, dwelt on the pathology of the affection, and showed that it was the result of morbid actions taking place within the eye, of an inflammatory nature, and mostly of an asthenic form. The retina and the choroid being but slightly connected, there was little or no resistance to extravasation, which quickly gravitated from one part to another. The separation of the retina after an accident he explained in the same way, there being first the inflammation and then the exudations between the retina and choroid. It was characteristic of the affection to proceed painlessly, and without any other symptom but that of impaired vision, the peculiarities of which were carefully pointed out. A remarkable case was given in illustration. A patient was brought to Mr. Walton with detached retina in one eye of old standing, and inflammation of the interior of the other eye-ball producing among its effects haziness of the vitreous humour, so that the fundus of the eye could not be seen. After general treatment the vitreous humour cleared, the shreds and floating particles in it gradually disappearing, and there was discovered partial detachment of the retina. With all this morbid action in the interior of the eye, there was not the slightest trace of disease in any of the external tissues. This was just the kind of case, Mr. Walton said, that he had been looking for. In the one eye certain conditions had been developed, that left, as one at least of its effects, a separated retina; in the other, was to be seen that intensity of internal action which, in all probability, was a parallel of what had taken place in the first, and which in the end separated the retina. He operated on both eyes, and evacuated the sub-retinal fluid. He found it necessary to operate a second time on the eye recently affected, at the interval of a month, having up to that period still continued the general treatment, which consisted of small doses of mercury with hyoscyamus, and a mixture of iodide of potassium

with cinchona. The result was, in the eye with the acute disease, the restoration of useful vision. In that which had been attacked two years before, no benefit ensued. Mr. Walton gave a detailed account of his method of operating, which was by puncturing the sclerotic and leaving the retina untouched, the fluid escaping between the sclerotic and conjunctiva. He assigned several reasons for preferring this to puncturing the retina with an endeavour to cause the effused fluid to escape in the vitreous humour, as practised by the Germans. He gave the general result of many cases in which he had adopted this simple treatment. In the mass of them no benefit resulted, and indeed he expected little, because they were chronic cases. In some recent cases, however, he had not the slightest doubt of having an amount of useful vision.

ART. 95.—*On Disconnexion of the Incus and Stapes ; its effect upon the Function of Hearing, and its Treatment.*

By JOSEPH TOYNBEE, F.R.S., Consulting Aural Surgeon to St. Mary's Hospital, &c.

(*Proceedings of the Royal Medico-Chirurgical Society ; Medical Press and Circular*, June 20, 1866.)

The author begins by some observations on the anatomy and physiology of the chain of bones. He gives an account of the tensor tympani ligament, whereby the membrana tympani and the chain of bones are kept in a naturally resilient state. And he then shows that the function of the chain of bones is twofold:—(1) to transmit sonorous vibrations from the drum to the expansions of the auditory nerve ; (2) to act as the analogue of the iris in the eye by adapting the labyrinth for the reception of sonorous vibrations having varying degrees of intensity. In proof of the first-named function, the experiments of MM. Sissajous and Dessains are cited, by which faint undulating lines were produced by a slender style attached to the base of the stapes during the vibration of the drum by sonorous undulations. In proof of the second function of the drum, the fact was cited that during the act of listening the stapedius muscle relaxes the membrana tympani and the membrane of the fenestra rotunda ; on the contrary, when a loud sound is expected, the tensor tympani muscle draws tense the membrana tympani and the membrane of the fenestra rotunda.

The pathological conditions alluded to in the paper are:—(1) simple disconnexion of the incus and stapes ; (2) disconnexion of the incus and stapes, the long process of the incus being absent.

1. The author shows that simple disconnexion of the stapes and incus, if attended with no other lesion, is not productive of any appreciable deafness, inasmuch as the tensor tympani ligament is able to keep the two bones in contact, and the action of the tympanic muscles is not interfered with. But if the membrana tympani or its ligament is relaxed, in addition to the disconnexion of the stapes and incus, then the function of hearing is interfered with, and often only to this extent,

that the patient can hear only when the voluntary act of listening is performed—that is to say, when by voluntary muscular effort the incus is held in contact with the stapes. In this class of cases, gentle pressure on the outer surface of the drum by any resilient body restores the natural power of hearing, and the distress produced by the necessity of constant listening is quite overcome.

2. But if the membrana tympani or its ligament is much relaxed, then no voluntary effort can bring the stapes and incus into contact, and great deafness is the result. This deafness is also remedied by the application of an artificial membrana tympani, which, gently pressing upon the outer part of the chain of bones, keeps the incus and stapes in contact.

3. This disconnexion of the incus and stapes also occurs in conjunction with partial or complete loss of the long process of the incus, the membrana tympani being entire. The treatment in this class of cases consists in pressing inwards the membrana tympani so as to place its inner surface in contact with the head of the stapes, and to retain the two structures in contact.

The lesions above referred to also take place when the membrana tympani is perforate. When there is disconnexion of the incus and stapes, together with a thickening of the mucous membrane or the ligaments of the articulation, the treatment consists in keeping up gentle pressure upon the outer surface of the long process of the incus; when the long process of the incus is absent, the pressure must be upon the head of the stapes. In order to exercise gentle pressure upon the ossicles and still to allow the muscles to move them, the author has recently suggested a new kind of artificial drum, in the shape of a small globe of india-rubber containing air.

ART. 96.—*Night Blindness.*

By J. W. HULKE, F.R.C.S.E, Assistant-Surgeon to the Middlesex Hospital and Royal London Ophthalmic Hospital.

(*Medical Times and Gazette*, June 16, 1866.)

Mr. Hulke records a few cases selected from his note-books for the purpose of illustrating the two conditions of which night-blindness is the common symptom.

The first is exhausted sensitiveness of the retina caused by prolonged exposure to glaring light, which makes the bacillary layer of the retina incapable of being sufficiently stimulated by twilight for perception.

This affection is common amongst sailors in tropical seas. Men suffering from it cannot see to do their work on deck after the sun goes down, except perhaps at full moon when the sky is clear; while in a well-lighted cabin their visual acuity is not much diminished. Mr. Hulke has examined many cases of this kind without finding any organic change appreciable with the ophthalmoscope. After a few days, or at most a

week or two, ashore, the night-blindness disappears, but subsequent attacks are not unusual when those who have once suffered are exposed to the same cause.

Rest and tonics comprise the treatment. The very frequent occurrence of night-blindness in men suffering from scurvy has led to the idea that here it may depend on purpura of the retina, but he has never yet seen this, though he has often looked for it in night-blind scurvy patients. Their liability proceeds from their feebleness, in which the retina shares.

The second condition of which night-blindness is a symptom is a contracted state of the visual field, depending on structural alterations in the choroid and retina, characterized by very definite ophthalmoscopic signs. The bottom of the eye is overlaid with dots and clusters of black pigment scattered and massed, and often grouped along the course of the retinal vessels. The pigment is a derivative of the choroidal epithelium, and lies on the surface of the choroid, and also among the withered retinal tissues. Between the clusters the epithelium is wasted, and the other tissues of the choroid are seen to be atrophied. These alterations progress from the front of the choroid and retina towards the optic nerve, and they are attended with a corresponding diminution of the area of the visual field, which becomes so small that the patient has not a general view of surrounding objects and stumbles over them at night; though so long as the centre of the retina remains unimplicated, the smallest type can be read in the contracted field. Ultimately, the optic nerve grows anæmic, the entire retina is involved in atrophy, and these final changes are marked by diminution of visual acuteness, ending in blindness.

The disease has been named *Retinitis Pigmentosa*, and the name may be retained if we remember that the primary seat of the affection is the choroid. Usually it begins in early life, and it runs a very slow course, protracted through many years. Liebreich, who devoted much time to its investigation, found that about half the sufferers from it were the children of blood-relations. Hitherto Mr. Hulke's own inquiries on this point have always met with a negative answer. The disease is frequently traceable to inherited syphilis, and it often affects several members in a family.

ART. 97.—*On Fractures of the Superior Maxilla.*

By Dr. A. GUÉRIN.

(*Archives Générales de Médecine*, Juillet, 1866.)

Dr. Guérin discusses this subject in reference to the displacement or not of the fractured bone, and he suggests a new method of distinguishing the fracture when displacement has not taken place. He holds that there exist fractures of the superior maxilla without displacement, and that in such cases mobility and crepitation are difficult to discover. But he considers that pain produced by pressure over the internal plate of the pterygoid apophyses is a pathognomonic sign. He believes also that it is more easy at the end of several days to discover the mobility of these apophyses than of those of the maxilla. Lastly,

he states that the ascending apophysis of the palatine bone is necessarily broken, and that he possesses a preparation showing that fracture of the vertical plate of the ethmoid occurs coincidently with fracture of the maxilla and pterygoid apophysis.

ART. 98.—*On Ulceration of the Internal Carotid consecutive to Caries of the Petrous portion of the Temporal Bone.*

By M. JACQUES JOLLY.

(*Archives Générales de Médecine*, Juillet, 1866.)

From an examination of this subject, M. Jolly concludes that:—

1. Ulceration of the internal carotid, consecutive to caries of the petrous portion of the temporal bone, is a very rare accident, in comparison with the great frequency of this malady. It is relatively frequent, on the contrary, in relation to the ulceration of other arteries.

2. Of all the vessels in the vicinity of the petrous portion of the temporal bone, this artery is most frequently injured.

3. The diagnosis of the perforated vessel is often difficult, but an attentive study of the hæmorrhages, their mode of production, and of the blood which is effused, will often be sufficient to remove any doubt; and in difficult cases the physician will derive useful information from compression of the carotid. If this should arrest the flow of blood, the hæmorrhage arises from the artery; if, on the contrary, the flow continues, uninfluenced, it arises from the veins.

4. The prognosis of this complication is always extremely serious, because it renders a grave surgical operation necessary which does not always succeed.

5. When ulceration of the internal carotid has been recognised, the surgeon has but one resource, ligature either of the internal or common carotid as early as possible.

ART. 99.—*A Case of Dislocation of the Jaw during Laryngoscopic Inspection.*

By Dr. GUIGNIER.

(*Union Médicale de la Gironde*; *Journal of Practical Medicine and Surgery*, 1866.)

One of Dr. Guignier's patients, a lady aged thirty-eight, suffering from tuberculosis and laryngeal ulceration, on two occasions, at a month's interval, dislocated her jaw, while Dr. Guignier was applying caustic to the glottis, the maxillary being completely luxated forward at the conclusion of the operation. The displacement was immediately reduced in the usual manner by downward pressure at the roots of the coronoid process behind the molar teeth.

This kind of luxation, which disagreeably impresses the patient and the persons present, deserves further notice. It can easily be prevented by recommending the subject to make but moderate exertions in the act of lowering and projecting forward the lower maxillary, during the introduction of the laryngoscope.

The mechanism of the displacement is apparently referable rather to the energetic contraction of the external pterygoids than to the exaggerated disjunction of the jaws; even when the mouth is moderately open enough for the purposes of laryngoscopic inspection, the articular condyles are already propelled forward, the external pterygoid acts powerfully on them, and easily draws them, in persons predisposed to the accident, over the transverse root of the zygomatic process.

This view would confirm the theory propounded by Boyer as to the active share of the external pterygoid muscle in the production of luxations of the lower jaw, and shows the error of the opinion entertained by J. L. Petit, who ascribed the displacement to spasm of the masseter, a muscle which is obviously unconcerned in the movements required by laryngoscopic inspection.

ART. 100.—*Case of Dislocation of Fifth Cervical Vertebra.*

By C. C. GRAY, Assistant-Surgeon U.S.A.

(*American Journal of the Medical Sciences*, July, 1866.)

Mr. Gray reports the following rare case:—"On the morning of Feb. 10th, 1866, I was called to see Private John Frank, Co. B, 2nd U. S. Infantry, who was reported 'badly hurt by a fall.' I found the patient, a muscular, powerful German of thirty-five, lying upon a table in the company kitchen; his face pale, respiration sighing, pulse slow and full. From himself, and from those about him, I gathered the following particulars relative to the accident: About ten minutes before he had invited the bystanders to witness a gymnastic feat.

"A few yards away the ground was thickly littered with short straw, which had been emptied from bedsacks. Starting towards this straw, he ran a few steps, and bounding two or three feet in the air, attempted to throw a somersault without touching hands or head. Although accustomed to perform this exploit, he from some cause failed on this occasion. Instead of alighting upon his feet, his head struck the earth or rather straw, and he rolled over upon his side, and lay motionless. As he did not arise, his comrades approached, and found him in the condition mentioned above.

"Upon examination, I found that sensation and power of motion were alike wanting from the neck downward. The walls of the chest were motionless, and respiration was effected by the diaphragm alone.

"He was unable to raise the head, but moved it freely from side to side. In attempting to examine the neck, it was necessary to lift the head from the table, which movement caused so much distress that I was obliged to desist. I, however, discovered—as I thought—a slight but unusual depression immediately below the spinous process of the fourth or fifth cervical vertebra.

"The patient was conveyed to the regimental hospital, and placed upon a hard mattress, all pillows having been removed.

"Assistant-Surgeon S. H. Horner, U. S. A., saw the case with me, and together we endeavoured to ascertain the nature and extent of the injury. By carefully supporting the head, the patient was turned partially upon his side, and a clear view obtained of the posterior parts of the neck. The examination was very unsatisfactory, for so thick were the layers of muscle and fat, that the usual landmarks, the spinous processes, were indistinct, and we were unable to arrive at a positive diagnosis. It was clear, however, that there was an abnormal gap or depression between the spinous processes of the fourth and fifth, or fifth and sixth, cervical vertebræ; that pressure at this point of depression gave slight pain; that there was an absence of crepitus, and that the movements of the head upon the atlas, and of the atlas upon the axis, were such as to prove that these articulations were not involved. Respiration indicated that the lesion, whatever its nature, was below the origin of the phrenic nerve, while the total paralysis of the upper extremities could not be explained on any other theory than that of injury higher than the origin of the brachial plexus. It was further agreed that we were not likely to benefit the patient by attempting to rectify a distortion concerning the nature of which we were ignorant. From this time forth he was accordingly undisturbed. My function consisted in directing such small attentions as were possible in the case, and in watching the process of dying.

"He lay, as before stated, perfectly supine, breathing by the diaphragm; suffered no pain, and was able to swallow small quantities of fluids. His pulse, which immediately after the accident had been 78, in two hours had fallen to 72. Respirations 20 per minute.

"Remained in this condition during the day. In the evening about three ounces of turbid urine drawn off by catheter. On the morning of the 11th, the pulse was 65; respirations 23 per minute. Somewhat drowsy and dull, but perfectly rational and cheerful. Countenance dusky from venous congestion. Liquid food had been twice administered. Urine (six ounces) again evacuated by catheter, and tested, giving a strong acid reaction. In evening pulse 62; respirations as before; face livid. Four ounces of urine evacuated and tested, giving same result as before.

"Died quietly at 6 A.M., February 12th, forty-four hours after injury.

"*Autopsy five hours after death.*—Rigor mortis imperfectly established; suggillation general over posterior portions of body; ulceration had already commenced over sacrum. The lower and back part of neck exhibited tumefaction—slight, yet sufficient to obliterate the depression which had been felt during life. The whole of the cervical portion of the spinal column was exposed by dissection, revealing a dislocation backwards of the fifth cervical vertebra. Both the superior articulating processes of this bone looked directly backwards, and its bifid spinous process was astride of and locked fast upon the neck of the spinous process of the sixth. So perfect was this impaction, that the spinous processes of the fifth and sixth could only be felt as one, until after all the soft tissues covering them had been dissected away. The luxation was 'symmetrical' in respect to lateral displacement. Of course, there

was a wide interval, one and a half inches, between the spines of the fourth and fifth vertebræ, which interval constituted the depression before mentioned.

"There was no fracture of the body, pedicles, or laminae of the displaced bone, but on the right a small fragment of the anterior tubercle of the transverse process had snapped off.

"The subflavian and capsular ligaments between the fourth and fifth vertebræ had given way, as had also the attachment of the ligamentum nuchæ to these bones. The anterior and posterior common ligaments were unruptured.

"There was a slight extravasation of blood external to the sheath of the spinal marrow, and a considerable quantity between the sheath and the cord. The upper and posterior edge of the fifth cervical vertebra encroached to such extent upon the spinal canal that the cord at this point was bent at an abrupt angle, and its antero-posterior diameter reduced more than half.

"The meninges of the cord were not torn, nor was the cord itself lacerated; which may perhaps be explained by the fact that the wide separation of the bones allowed it (the cord) to bulge out posteriorly, and thus escape.

"The lungs were congested, but crepitant throughout; the air-passages filled with frothy mucus; the heart healthy, empty, and well contracted.

"The cervical vertebræ were removed entire; the dislocation unreduced. The specimen has been deposited in the Army Medical Museum (Museum Number 549).

"I have been unable to find a record of a similar case. So unique a dislocation could only result from a very complex 'composition of force and resistance.'"

ART. 101.—*Case of Excision of a part of the Spinal Accessory Nerve for Spasmodic Wry Neck.*

By Mr. CAMPBELL DE MORGAN, F.R.S., Surgeon to the Middlesex Hospital, Examiner in Surgery to the Royal College of Physicians.

(*British and Foreign Medico-Chirurgical Review*, July, 1866)

A healthy labouring man, æt. thirty-two, living in the country, was crushed down by the weight of a heavy ladder which he was attempting to lower. His neck was bent under it, but no particular injury appeared to have been done, and he paid small attention to the accident. This occurred in October, 1860. Nearly two months after, in December, he became affected with twitchings in the neck. He thinks, however, that for some months before the accident there had at times been a tendency to jerking of the neck to one side.

These spasmodic attacks, at first occasional, rapidly became more powerful and continuous, so as, in the course of two or three weeks, to

thoroughly unfit him for work. On the 19th January, 1861, he was admitted into the Middlesex Hospital.

His appearance was peculiar. There was an anxious, worn look in the countenance, which at times changed to a sort of sardonic smile, from spasm of the facial muscles. The eyes were constantly twitched towards the right. He could fix them by an effort for a short time, but the twitching soon recurred. The head was spasmodically drawn to the right side, and the right shoulder was at the same time raised towards it. There was with this movement a rotation of the head, the chin being turned towards the point of the right shoulder, with the face looking directly over it. The spasms were at times so violent as to draw the chin behind the line of the shoulder. The sterno-mastoid and trapezius muscles were thrown into strong relief during the more violent spasms. The right shoulder was always on a higher level than the left, and this gave an appearance of distortion to the body, but the spine was quite straight.

Although the sterno-cleido-mastoid and trapezius muscles were apparently the seat of the most violent spasm, yet it was evident from the position of the head that their action was not the sole cause of the distortion. The combined action of these muscles would tend to bring the head down towards the shoulder, and to raise the shoulder itself, but at the same time to turn the chin towards the opposite side. The great pain which he suffered and the spasmodic contractions were due, probably, to the antagonistic action of several muscles—the splenius, and the inferior oblique and the greater posterior rectus dragging the face round in opposition to the actions of the trapezius and sterno-cleido-mastoid. There was no affection of the muscles of mastication.

By a very strong effort, and aided by the pressure of his hands, he could nearly, but not quite, bring the head into its natural position; but this was in a few seconds followed by more severe spasms. Any attempt by others to restore the head to its position by external force gave rise to such violent muscular action in the neck as to make it insupportable. When the paroxysms were severe he suffered very great pain, and he was never altogether free from discomfort.

During sleep the head was sometimes, though rarely, quiet, and lay in a natural position; but generally it was twisted round, and at times the spasms came on so as to awaken him. Sometimes he was altogether prevented from sleeping by them.

There was no appearance of disease or injury about the spine; the examination would bring on more powerful action, and thus produce pain; but the same would occur if any part of the right side of the neck were handled. He complained of pain down the back; but there was no particular tenderness in any part of it. His general health was impaired by the constant pain and loss of rest.

The most careful examination failed to reveal any special point of irritation which might by reflex action give rise to these spasms. Mr. De Morgan's impression was, that they had a deep origin—the spinal accessory nerve, the abducens oculi, and some of the branches of the first and second cervical nerves being principally involved in their production. Whether they were reflex actions from some deep-seated irritation, perhaps within the spinal canal, or were set up by direct

irritation in the nervous centres, could not be determined; the former view seemed the more probable.

For many months he was subjected to treatment, local and general, but with no benefit. Counter-irritation to the neck and over the spine generally, ice, and heat to the spine, galvanism, electricity, the local application of belladonna, opium, veratrine, and such like agents; the internal use of a host of sedatives, antispasmodics and alteratives, were alike impotent. The sub-cutaneous injection of morphia certainly relieved him and procured sleep; but he was not essentially better after a prolonged trial of it. Chloroform readily affected him, and under its influence the spasms entirely gave way, but they returned with all their former violence when its effects had passed off.

Then Mr. De Morgan determined to divide the sterno-cleido-mastoid muscle. It was not a case in which the same benefit could be expected from the operation as in ordinary wry neck. But one often sees that when a large class of muscles is affected by spasm induced by local irritation, relief is given to all by the section of one of the antagonistic muscles chiefly involved. The operation was done by making a puncture at the inner side of the sternal tendon of the muscle, about a quarter of an inch from its origin, carefully carrying a blunt-ended tenotomy knife flat along the posterior surface of the muscle, feeling the resistance of its fibres the whole way, and then, turning the sharp edge towards the muscle, and rendering the fibres as tense as possible, cutting entirely through it. No bleeding took place. The extreme tension and spasm were at once markedly relaxed, but by no means entirely overcome. After twenty-four hours an attempt was made to keep the head in a more natural position by means of a collar constructed for the purpose; but although it could be brought into position with much less difficulty than before the operation, and could be fixed in it by the collar, the spasms were yet strong enough to drag the head round towards the shoulder, and the pain from the resistance of the collar was too severe to be long sustained. This treatment was soon discontinued, as it evidently did harm.

The muscle united quickly, and the spasms recurred with as much violence as before.

The man's health was giving way under the constant pain and irritation, and it was evident that it must eventually break down altogether unless some decided relief could be obtained. The sterno-cleido-mastoid and trapezius muscles were clearly exercising a powerful traction on the head, and Mr. De Morgan thought that if their united action could be prevented, that of the antagonistic muscles, even if persistent, might be controlled or tolerated. He was encouraged in this opinion by having seen the relief which the division of the sterno-mastoid alone afforded to the patient.

The division of the external branch of the spinal accessory, and the removal of a part of the nerve, seemed alone to promise the desired effect, and this operation was performed in February, 1862.

An incision two inches long was made along the posterior border of the muscle, the centre of the incision corresponding to about the centre of its edge. The fascia being slit up to the same extent, the trapezial branch of the nerve was sought for as it emerges from the sterno-cleido-

mastoid to cross the posterior triangle of the neck. It would be found generally a little above the centre of the incision. When found, the nerve was traced through the fibres of the muscle—the fibres being cut through much as is done in an ordinary anatomical dissection—until the common trunk above the division into the trapezial and sterno-mastoid branches was reached, and here a piece about a quarter of an inch in length was cut out. As the operation was, of course, done under chloroform, no effect was observable when the nerve was divided, the muscles were already thoroughly relaxed from its influence. On his recovery from the effects of the chloroform the trapezius and sterno-cleido-mastoid were found to be completely paralysed, and although there was still an occasional and slight convulsive movement of rotation of the head, it lay on the pillow in almost a natural position. There was no tendency whatever to undue action of the corresponding muscles on the opposite side. The respiration was not in any way affected, nor did he experience any peculiar sensation. All he did feel was relief from the extreme tension of the neck. The countenance was more tranquil than it had been for months. The wound healed without any trouble.

When he got up it was found that the head maintained nearly its natural position. He did not require any special support. There was still some slight action of the rotatory muscles of the head; the sterno-cleido-mastoid and trapezius remained perfectly flaccid, except at the back and upper part of the clavicular portion of the former muscle, which was tense, and evidently acted when he attempted to bring the right ear down towards the shoulder.

He soon began to regain flesh and strength. He was kept in the hospital for three months after the operation, and he was discharged in May, 1862, having been in the hospital upwards of sixteen months. On leaving the hospital he went down to the country, where he was soon able to resume his work as a labourer and thatcher.

Mr. De Morgan heard of him from time to time, and in January, 1865, he sent for him to town in order to examine into his condition. He was looking healthy, the countenance was tranquil, the face turned directly forward, with the forehead and chin in a perpendicular line. Occasionally and for a few seconds there was a trifling twitch of the head towards the right side, with a little movement in the eyes. Any sudden touch or excitement would bring this on. The right arm hung listlessly against the side. The body was a little deflected from the perpendicular, so that a line dropped from the centre of the forehead fell an inch and a half to the left of the pubic symphysis. This was owing to a uniform and very trifling arching of the spinal column, the concavity being directed towards the left; there was no indication of a double curvature. The right shoulder and right nipple were about an inch higher than the left. This gave an appearance of increased size to the right side of the chest, but the measurements were the same on the two sides. The right shoulder, however, projected more from the right side than the left. Measured either from the vertebra prominens or from the centre of the sternum, there was a good inch more of length to the tip of the acromion on the right than on the left side. This was due to the right shoulder being brought more to the horizontal

position, while in the left shoulder the slope was perhaps greater than natural.

The right sterno-cleido-mastoid muscle was completely wasted, except at its upper and posterior part; here for about the breadth of half an inch, and extending from behind the mastoid process to the middle of the posterior border of the muscle, it was nearly as large as on the opposite side. Towards its lower end this band of fibres, which contracted strongly on his moving his head, tapered off to a point.

The trapezius was entirely wasted; a lamina not thicker than a shilling, and quite flaccid, could be felt in the neck. No contraction could be discovered in any part on his moving his head or shoulders. The rhomboid muscles could be seen in action below its dorsal part. These muscles were, Mr. De Morgan thinks, larger than natural. On the opposite side the trapezius was largely developed. On his raising the shoulders the right was elevated by three quarters of an inch more than the left, and, although the trapezius was so wasted, the right shoulder when raised appeared fuller than the left.

There was a little rotation of the right scapula, the inferior angle being tilted upwards and outwards, and the outer angle forwards.

The right arm and forearm were as powerfully developed as the left; the deltoids were equal on the two sides, and no difference was observable between the two great serrati.

His respiration was natural, and nothing peculiar was observed on his making a forced inspiration.

No alteration of sensibility was to be discovered in the neck and back.

This is, probably, the only instance of resection of the trunk of the external branch of the spinal accessory, and it is consequently interesting in a physiological as well as in a surgical point of view.

The fact of the upper and posterior part of the sterno-cleido-mastoid muscle retaining its activity may be accounted for in two ways. First, by the existence of some twigs given off from the nerve to the muscle before its division into its two main branches. This is unusual, but is not improbably the real solution. The second explanation is, that as the nerve forms numerous connexions with the cervical nerves, some of the branches may have supplied this part of the muscle. This is rendered improbable by the interesting fact that although both the sterno-cleido-mastoid and the trapezial branches of the spinal accessory are freely associated with the cervical nerves, every part of both these muscles, with the exception above noticed, was completely paralysed. The muscles acted neither by volition nor in respiration.

The rotation and elevation of the scapula were probably due to the action of the serratus magnus—unbalanced by the trapezius—but in some degree limited by the increased action of the rhomboids. The rhomboid muscle would prevent the serratus from drawing the scapula too much forwards, but would tend at the same time to elevate it; and the serratus magnus itself would, I believe, raise the outer angle of the scapula, as well as bring it forward if uncontrolled by the trapezius.

The position of the body was a natural result of that of the shoulder. As the axis of the right shoulder from the spine to the acromion was an inch longer than that on the left side, the arm would hang at the end of a longer lever. To compensate for this the body would necessarily be

somewhat arched to the opposite side, as is done whenever the arm is kept extended.

In a surgical point of view, the case is of interest as one of unusual severity and involving a large class of muscles. The pathology of wry neck from muscular action is but imperfectly understood. Mr. De Morgan's belief is, that the complaint is due to an irritation of the nerves in every instance in which inflammation or some disease of the muscles themselves has not preceded it. He doubts whether, as is often alleged to be the case, it is ever caused by paralysis of the muscles of the opposite side.

What ground was there, then, for believing that by paralysing one set only of the spasmodically affected muscles relief would be given to the spasms in the other? Had the spasms had a central origin in the cord, it is doubtful that much benefit would have followed the operation. But there was the evidence that the symptoms were partially relieved so long as the sterno-mastoid remained ununited after its division. And it is well known that where a large class of muscles is involved in spasmodic action, arising from reflex irritation, the section of a single muscle will entirely remove it.

(B) CONCERNING THE TRUNK.

ART. 102.—*On Contraction of the Anus, and forcible Dilatation.*

By Professor ROSSANDER.

(*Hygieia*, 1865 ; *Schmidt's Jahrbücher*, 1866.)

It is well known that the first description of the contracted or fissured anus was given by Boyer, who also devised an effectual plan of treatment. Subsequent writers have studied the ætiology and the nature of the malady, and have sought for a milder method of cure. For this purpose it has been essential to determine what was the first step in the disorder, whether the contraction was the cause of the fissure, or the result of reflex irritation proceeding from it. In other words, to which of the principal features of the disease the surgeon should direct his attention?

Prof. Rossander states that, according to his experience, the malady is more frequent than is commonly supposed; and that it is not seldom the cause of symptoms that are attributed to other conditions. During the decade 1853-63, fifty patients suffering from it came for operation into hospital, under the care of the author and his colleague, Prof. Santesson; and, in private practice, the author saw eight cases in 1864. Of the hospital patients twenty-four were males and twenty-six females; but the statement of most writers that females greatly preponderate (among 150 cases, treated by Maisonneuve, 120 were in women) agrees with the author's experience in private practice. Of the last eleven cases treated by him, nine were in women. Most of the patients were between twenty and forty years old; but in private practice the author

operated upon a woman sixty-five years old; and, in hospital, upon a child of four years. That the malady may occur in young children, and may even be congenital, had already been shown by Boyer; and Prof. Abelin has found fissured anus with contraction as well as contraction without fissure, in sucking infants as well as in older children.

The causes of the disease are not easily determined, although most authors are agreed that it commonly attacks persons who are habitually constipated. Rossander thinks this statement goes too far, and confounds an effect with the cause. The disease infallibly produces constipation; and, when the patient has thus suffered for several years, he is often unable to say positively what was the earliest symptom. It cannot be denied, however, that difficult defecation may produce fissure in various ways; among others, by laceration of the mucous membrane by scybalæ. But this occurs less frequently than those assume who regard the fissure as the first and essential phenomenon of the disease: since very large and hard fæcal masses will pass, without doing the smallest injury, through an anus that is not morbidly contracted; and of all the patients seen by the author, only one declared that the pain had commenced suddenly. When, however, contraction of the sphincter is already present, the formation of fissures by hard fæces may happen very easily. Habitual constipation may also occasion the disease indirectly, by its influence upon the portal circulation, giving rise to hæmorrhoids and varicose vessels in the neighbourhood of the sphincter, and thus causing irritation which may excite in that muscle a similar reflected contraction or spasm. All circumstances, indeed, that tend to congestion of the anal or perineal region, are liable to occasion contraction. Hence the disease often commences in the later periods of pregnancy. Tumours of other kinds in the abdomen or pelvis, and any sources of irritation about the genito-urinary organs, may act in a similar manner. Lastly, contraction may be excited by injuries and other local causes, among which must be reckoned anal ulcers, whether they be fissures or not.

The subjective symptoms depend chiefly on the pain in defecation; pain that is at first inconsiderable and transitory, but that becomes more severe and of longer duration. Those who maintain that the whole disorder is dependent upon an anal fissure, describe as the first symptom a penetrating and burning sensation in a limited and determinate part of the anal circle, but this has never been observed by the author. Purgatives are commonly much used by the patients, either to soften the evacuations or because without them there would be no evacuations at all. At first they diminish the pain, but not subsequently, and a loose motion becomes as tormenting as a hard one. If the disease be complicated by internal piles, these will be forced out during evacuation, and then constricted by the sphincter; and their return, always painful, will at last become scarcely possible. In such cases there is often considerable bleeding during evacuation; but otherwise the stools are usually less bloody than they have been described. The pain radiates in various directions; but the genito-urinary system is usually the first to share in it. There is often spasm of the bladder with frequent and painful micturition; and these symptoms may be sufficiently severe to divert the attention of both surgeon and patient from

the original malady. Coitus may also become painful, especially to women, and so also may menstruation. Rossander found contraction of the anus in a young lady who consulted him on account of cramp in the calves of her legs; but she refused to submit to operation, so that he could not determine that the contraction was the cause of the cramp. Lastly, many of the so-called nervous diseases, such as hypochondria, hysteria, cardialgia, and others, are found among the consequences of contraction; while the author has never seen them associated with fistula.

Among the objective symptoms the contraction of the anus is the most constant. Even from outside, the sphincter may be felt as a hard ring, and the finger introduced into the bowel is met by considerable resistance, and occasions the patient the most acute pain. The breadth of the hard circle by which the finger is constricted is often only a few lines, but is sometimes much more.

Occasionally two rings are felt, one above another. The author has not observed the general flattening of the whole anal region that has been described by others, but, on the contrary, has found it strongly wrinkled, so that the folds of skin and venous membrane were deeper than usual. There will often be one or more ulcers, commonly elongated in shape. The edges of these ulcers are hard, their surfaces velvety, but only visible at the lower part. The ulcers may be more or less deep, are often situated posteriorly, not exactly in the middle line; and when the folds in which they are situated are stretched out, are found to possess a round or an oval form. Sometimes the fissure is first discovered when the patient presses forward the folds of the mucous membrane by straining. Sometimes the most careful examination discovers no fissure, or only an excoriation of some fold of skin. Such excoriations are produced in the more or less wrinkled and inflamed mucous membrane by small portions of excrement, left unremoved by the patient on account of the sensitiveness of the part.

It is impossible, according to Rossander, to consider these small excoriations, present one day and healed the next, as the source and origin of the disease. The essential morbid phenomenon is spasm of the sphincter, one of the causes of which, perhaps even the most common cause, may be fissure; but still fissure alone does not constitute the disease. There are many instances of anal ulcer (*e.g.*, syphilitic ulcers) in which there is no pain on evacuation. It is necessary to cure the spasm; and it is a matter of indifference whether a fissure co-exist or not. This opinion is best confirmed by the results of treatment, which furnish the strongest argument in favour of Boyer's views. The author states that when he is consulted by a patient who complains of the characteristic pain, and in whom the sphincter is strongly drawn together, he does not endeavour to ascertain whether a fissure exists; since the examination would only give the patient unnecessary pain. The use of a speculum is for the same reasons objectionable. The finger should be introduced in order to determine the existence of contraction, its extent, the state of the intestine and mucous membrane above the sphincter, and to discover the evidence of any tumour that might require removal. It should not be moved to and fro, in order to ascertain whether any one point of the anal circle was especially painful.

Although the diagnosis is generally easy, on account of the remarkable character of the symptoms, yet the author is of opinion that these symptoms are very often overlooked. It is common to meet with patients who have believed their sufferings to be due to piles, and who, in that belief, have employed a variety of remedies without consulting a surgeon; or who, especially women, have refused to submit to any proper examination.

To confound this disease with others would not be easy. The subjective symptoms of cancer of the rectum resemble it in some degree; but with ordinary attention the two can readily be distinguished. Hæmorrhoidic tumours may simulate contraction; but the two conditions frequently co-exist, and in special cases there could be no difficulty in determining whether the latter was present or not. The prognosis, if the disease be left to itself, is always unfavourable. The author thinks it possible that actual contraction may disappear after the removal of its exciting cause; but such cases would be difficult to recognise. When the disease is treated, the prognosis is especially favourable, there being few surgical operations so certainly curative and attended with so little danger as those for anal contraction, especially forced dilatation. Although some surgeons profess to effect a cure by various ointments, local applications, and laxatives, yet the treatment is essentially operative. The author does not advise immediate operation in every case, and thinks that, when contraction is excited by a recent fissure, other means, especially cauterization with nitrate of silver, may be of service. With regard to the ointments, he thinks that their liberal application, and introduction within the sphincter, may produce some degree of dilatation.

Among operative procedures that by incision is the oldest and best established, and Boyer believed that by this means he had radically cured all his cases. The author has never himself employed the large and deep incision recommended by Boyer; but he holds that Boyer's statements are worthy of all credit. Many other surgeons have failed to obtain good results by incision; and it is possible that their incisions may not have been deep enough to relieve the spasm. Some have observed evil results from the incision, whence Vidal, Dupuytren, Guérin, and others, have modified Boyer's operation in various ways. The author describes several of these modifications; and, among all of them, gives the preference to Blandin's suggestion of a subcutaneous section. This is, however, difficult of accomplishment, and has been little practised, since the same end may be attained more easily by the method of *forcible dilatation*. Soon after Boyer's discovery of the nature of the disease, it was suggested that dilatation might afford a remedy. Dubois, Marjolin, and others sought to attain this end by a succession of tents, lubricated with anodyne ointments. Velpeau advised that the first dilatation should be complete, without regard to pain. For this purpose the author recommends the procedure of Maisonneuve, known as "forcible dilatation." On the day before the operation the patient should take a purgative of castor-oil, and on the morning of the operation an enema, so that the bowels may be undisturbed for a period of from twenty-four to forty-eight hours. The patient is placed upon his side and chloroformed, both to diminish pain and to facilitate dilatation. Many patients,

however, and especially ladies, have refused chloroform, because they feared it more than the pain, and because they objected to the presence of a second surgeon. The operation is commenced by introducing one well-oiled index finger with its volar side towards the os coccygis, and the other exactly opposite to it, so that the backs of the two fingers are in contact. They are so bent that the third phalanx of each forms a right angle with the second, and is carried completely above the ring of muscle. The dilatation is then commenced by pulling with the two fingers directly forwards and backwards, and the distance between them is gradually increased, until the sphincter is stretched as much as possible. The muscular fibres will be felt to yield, and some of them may perhaps be torn, but this is of no consequence. In order to obtain a good result considerable force is necessary, and the distance between the backs of the two fingers should be at least an inch, often an inch and a half, at the end of the operation.

The anus remains for a short period wide open; but after a few minutes it closes again. The little rents or fissures made in the mucous membrane, and the trifling bleeding from them, as well as the ecchymosis produced in the anal region, are of no consequence, and require no treatment. Pain commonly continues for some hours, and is sometimes very severe. Usually it is not worse, and is more transient, than that which has followed every evacuation; often it is much less severe. Frequently there has been some difficulty in micturition, not continuing after the second day. After simple dilatation, Rossander has never seen any abscess or other unfavourable complication. The paralysis of the sphincter is very temporary, and has never given rise to incontinence of fæces. Indeed, the effects of the operation were so slight, that many persons requiring it have been treated at the hospital as out-patients, and allowed to proceed to their homes after a short repose. As a dressing, nothing is required but cold compresses, renewed as often as they become warm, so long as the pain continues. Among all the cases treated at Stockholm there has been no relapse; and, commonly, the first stool after the operation (especially when delayed for forty-eight hours or more), and always the second or third, has been wholly free from pain. With the removal of the pain, all secondary or sympathetic sufferings have also disappeared.

The various methods of operative treatment that have been advised for contraction of the anus may all cure the disease, but none of them so speedily, certainly, and safely, as dilatation. The small incisions are often insufficient. The deep incision of Boyer is equally effectual and often less painful. But it has been followed by abscess, and even by death, and the resulting wound is always many weeks in healing. Blaudin's subcutaneous incision is less dangerous than Boyer's; but it is very difficult of accomplishment, and requires a special apparatus. The dilatation also is advantageous on account of the facilities it affords for the ligature or removal of piles, which very often complicate the original disorder, and which may be treated with great facility as soon as the sphincter has been stretched.

ART. 103.—*Simple Method of Radically Curing Reducible Hernia.*

By JULIAN J. CHISOLM, M.D., Professor of Surgery in the
Medical College of South Carolina, U.S.A.

(*The Lancet*, September 1, 1866.)

A simple plan for radically curing hernia, which Dr. Chisolm suggested and put into successful practice in 1859, consists in sewing the columns of the inguinal ring together, subcutaneously, by silver wire, and leaving the wire permanently in the tissues, so as to act the part of a permanent internal clamp. This restores to a great extent the virgin condition of the external oblique tendon which gives strength and support to the lower portion of the abdomen. The only instrument necessary for the performance of this operation is a stiff needle five inches in length, very slightly curved towards its point, near which is placed the eye. The other extremity of the needle is secured in a firm handle, which enables the surgeon to control its movements.

The various steps of the operation are as follows:—The patient having undergone the usual preparation of having the bowels emptied by some mild cathartic, is placed in the recumbent posture, and all hair is removed from the pubic region corresponding to the side upon which the operation is to be performed. The hernial contents having been returned into the peritoneal cavity, the index-finger of the left hand is placed over the centre of the fundus of the scrotum (palmar surface upwards), the needle lying upon and parallel with it, the eye of the needle corresponding with the pulp of the finger, which can guide it in the direction it should take to the point of transfixion. The finger, with the needle now capped by scrotal tissue, is passed up into the inguinal canal until the inner face of the columnus can be readily felt. The pulp of the finger having passed well behind the internal column, the handle of the needle is seized, and the point, directed by the finger, is made to transfix the conjoined tendon and internal column at some distance from its free border. When the point of the needle projects under the skin of the abdomen, an assistant draws the skin inward towards the median line, so as to make the needle perforate that portion of skin which would normally lie over the central portion of the canal. The needle is now threaded with a silver wire and then drawn back into the canal and through the scrotum, leaving one end of the wire exposed upon the abdomen. If the point of the needle has escaped from the scrotal puncture, it is carefully reinserted through the same orifice, and, directed as before upon the pulp of the finger, passes with the invaginated scrotum into the canal, and is made to transfix the external pillar of the ring. As the point lifts the skin the abdominal covering is drawn outwards in such a way that the point of the needle protrudes through the puncture first made in the skin of the abdomen. The silver wire is now detached from the eye, and the needle completely withdrawn through the scrotum, leaving the two ends of the silver

wire protruding from the abdominal puncture. The portion of wire embedded in the tissues forms a long loop, which extends continuously through each column of the ring to the bottom of the scrotum; the extreme convexity of the loop lying in the scrotal fascia under the skin, where it can be felt by passing a probe into the scrotal puncture.

The next step of the operation consists in drawing firmly upon the ends of the wire, whilst the scrotum is drawn downwards and its invagination prevented, which forces the wire to tear or dissect up the scrotal fascia to the immediate vicinity of the ring. If the finger be now thrust up into the canal and the wire drawn upon, the finger will be squeezed by the approaching columns; and if drawn out of the canal, and the wire be still drawn upon, the ring will be so diminished in size as only to accommodate the spermatic cord, with no room to re-admit the finger. The wire is now twisted from above with a torsion forceps, and when the columns are brought well in apposition, without too much traction being made to cause the wire to act as an *écraseur*, the ends of the wire are cut off as close as possible to the abdomen, when the portion left in the wound immediately disappears from view, under the skin.

From the beginning to the end of the operation not a drop of blood is drawn, the only external evidence of an operation having been performed being a small prick in the skin of the abdomen and a similar one in the scrotum, either of which can scarcely be found, and which heal in a few hours. For a few days after the operation the patient is kept quiet, until the wire can become imbedded in lymphic effusion. No truss need afterwards be worn, as the wire clasping the columns of the ring restores the support of the abdominal wall; the truss, moreover, would act injuriously by painfully compressing the skin against the incarcerated wire suture. One suture suffices for the majority of herniæ. Should the orifice of protrusion be of large size, as in large inguinal or umbilical herniæ, two or more sutures may be required to keep the borders of the opening in perfect apposition. A point of much importance is that of introducing the needle the second time exactly through the same orifice in the scrotal skin as it had traversed in its first introduction; for should a portion of the skin be involved in the loop of the wire, the ready dissection of the scrotal fascia cannot be effected without much force, and the scrotum becomes invaginated in the inguinal canal. The same rule holds good for the abdominal puncture, otherwise the twisted wire will not slip under the skin and become embedded in the subcutaneous fascia.

ART. 104—*A Case in which a New Operation for the Radical Cure of Hernia was successfully performed.*

By MR. ARTHUR E. DURHAM, F.R.C.S., Assistant Surgeon to, and Lecturer on Anatomy at, Guy's Hospital.

(*Proceedings of the Royal Medico-Chirurgical Society; The Lancet*, June 16, 1866.)

Stephen H——, a sailor, twenty-six years of age, sought admission to Guy's Hospital for the purpose of being radically cured of an easily-

reducible but very troublesome inguino-scrotal hernia on the right side. The hernia had been first noticed six years previously. It had gradually increased in size, and extended into the scrotum. Latterly it had given rise to such constant inconvenience and so much occasional pain that the patient had been quite unable to follow his occupation. He could not wear a truss, although he had repeatedly attempted to do so. At the period of admission to the hospital the scrotal portion of the hernia was about as large as a hen's egg, or rather larger. The bowel, of which it was evident the hernia mainly consisted, was easily returned into the abdomen, but a slight fulness of the inguinal canal persistently remained.

On Jan. 19th, chloroform having been fully administered, and the hernia reduced as completely as possible, Mr. Durham proceeded to operate in the following manner. An incision about two inches and a half in length was made through the skin and superficial fascia, in a direction at right angles to Poupart's ligament, and just over the inner border of the internal or deep abdominal ring. The tendon of the external abdominal oblique muscle was next divided in a similar direction, but to a somewhat less extent, and in a situation slightly further from the median line of the body. The lower fibres of the internal abdominal oblique or cremaster were then separated longitudinally, and the internal spermatic fascia, or fascia propria of the hernia, was exposed. A slight incision having been made in the lower and deeper part of this fascia, an aneurism needle was carefully insinuated through the areolar tissue, and by its means a ligature was placed between the sac of the hernia and the important structures of the spermatic cord, and carried through the upper and deeper part of the fascia. The fascia and sac were then drawn gently downwards and towards the median line of the body, and the ligature was tied tightly as high up and as far outwards as possible; in fact, as nearly as could be judged, exactly at the internal or deep ring. The ligature thus included the greater part of the circumference of the fascia propria or internal spermatic fascia just where it becomes continuous with the fascia transversalis, the whole circumference of the sac just at its junction with the general peritoneal lining of the abdominal parietes, and within the sac a small plug-like portion of omentum. In passing the aneurism needle, a slight puncture was unintentionally made into the sac. This puncture, however, when subsequently dilated, afforded the opportunity of ascertaining that the sac did not communicate with the tunica vaginalis testis, but that it contained a small piece of irreducible omentum; this was drawn out and cut off below the ligature. Finally, the wound was closed above and below by sutures, which were passed through the sac. The ends of the ligature were left coming through the middle of the wound.

The after progress of the case was most satisfactory. The patient was kept absolutely in the recumbent position for more than six weeks. From first to last he never had a single bad symptom worth mentioning. There was never any abdominal tenderness, constitutional disturbance, or other indication of general peritonitis; nor was there ever much pain about the site of the operation. The upper and lower parts of the wound healed by first intention. The ligature came away on the eighteenth day, and complete closure of the wound speedily followed.

On the 29th March, the patient, having to a certain extent recovered his strength, went into the country. Before leaving the hospital he was examined by all the members of the surgical staff as well as by many visitors. There could not be discovered the slightest hernial protrusion, nor any abnormal impulse on coughing. The inguinal canal from the internal ring seemed perfectly blocked by the obliterated sac and new material.

This case, although a solitary one, may be considered to prove—first, that the method of operation described is practicable; and, secondly, that it is not necessarily attended by danger. The author would add that he believes it to be more scientific in principle than any other method yet adopted, and he hopes therefore it may prove more successful in practice.

ART. 105.—*On the best form of Truss for Umbilical Hernia.*

By M. DEMARQUAY.

(*Union Médicale*, No. 33; *British and Foreign Medico-Chirurgical Review*, July, 1866.)

The best form of truss for umbilical hernia, according to M. Demarquay, consists in a little pyramidical pad formed of very supple vulcanized caoutchouc, and filled with air. The hernia in children can, by the aid of this, be easily retained. When it has to be applied it is affixed to a very supple strip of diachylon, as broad as itself and nearly long enough to surround the body. It should be renewed every day or two after placing the child in a bath, but it can be dried again for future use, and is very cheap. When at the end of some months the hernia has become very small, or has no longer a tendency to come out, the caoutchouc may be replaced by a little ball of wadding. These air caoutchouc pads may also, when properly adapted in size and form, be very advantageously employed in the adult, affording complete relief to the distressing symptoms induced when the hernia is ill-supported. The diachylon strip requires to be proportionally broader, and two smaller strips should lap over it above and below the pad, to keep it *in situ*.

ART. 106.—*Traumatic Destruction of the Posterior Part of the Urethra. Restoration of continuity. Cure.*

By Dr. NOTTA.

(*L'Union*, 1864; *Schmidt's Jahrbücher*, 1866.)

Dr. Notta records the case of a man who received several injuries by being buried under some fallen rubbish. Among them was a wound in the perineum, completely dividing the urethra. The first attempts to pass a catheter were unsuccessful; but, after a time, on opening an

abscess behind the symphysis, the author felt a somewhat depressed spot, and on introducing here a hollow sound, obtained proof, from the outflow of urine, that he had reached the bladder. He then introduced at the meatus a catheter of vulcanized india-rubber open at the extremity, and brought it into the wound. Through this catheter he carried a whalebone stilet; and, when this appeared in the wound, guided it along the hollow sound into the bladder. The sound was then withdrawn, and the catheter guided over the stilet into the bladder. After two months it was withdrawn, neither incrustated nor otherwise injured, on which account the author greatly recommends vulcanized india-rubber as a material for catheters. The second time the catheter remained in the bladder for eighty-two days, and, during this time, the perineal wound contracted and closed by granulations.

When the healing was complete, the penis was somewhat shortened: a natural result of the loss of substance. There was no difficulty in micturition, nor in the introduction of a full-sized catheter, which was practised from time to time as a precaution.

ART. 107.—*On the Treatment of Severe Stricture of the Urethra by Over-Distension.*

By MR. HENRY THOMPSON, F.R.C.S., Surgeon-Extraordinary to H.M. the King of the Belgians, Surgeon to University College Hospital, &c.

(*The Lancet*, June 6, 1866.)

Mr. Thompson first insists on the following fact, namely, *that that portion of the urethra which is most frequently affected by stricture possesses in its natural condition a calibre at least double that of the external meatus.* It follows then of necessity, he argues, that any kind of dilatation of the strictured portion which is limited in extent by the size of the external meatus does but half restore the natural calibre of the canal. In other words, the external meatus having in any case a diameter equal to that of No. 12 of our catheter scale, the natural diameter of the bulbous portion will equal at least that of No. 16 or of No. 18—an area more than double that of the former. Hence, if this latter portion becomes the subject of stricture, and the dilatation is carried no higher than No. 11 or 12, the stricture is not more than half dilated.

Practically, for the great majority of cases, such dilatation suffices for all purposes. The patient loses all his symptoms, and continues well on condition that he keeps open his stricture by occasionally passing an instrument. Happily, ordinary dilatation effects all this for most patients. For a few exceptional cases, however, it is insufficient to relieve the symptoms. The passing of a full-sized instrument does not enable the patient to pass even a tolerable stream; it often produces absolute retention, or rigors, or increased pain. For such a stricture we are accustomed to use some more efficient proceeding—*e. g.*, to divide

the hardened tissues (urethrotomy), or to burst them at a single operation (rupture). Each proceeding has been, and is, of considerable value when judiciously adapted to the case.

But by neither of these operations is the urethra restored to its original calibre at the point of stricture. So long as the rupturing instrument or the dilating instrument is limited in its size by the calibre of the external meatus, the contracted portion of the urethra is only half dilated.

Referring to the treatment by incision, Mr. Thompson says that in many obstinate cases a perfect result may be accomplished by means of a powerful distending instrument without any incisions at all. The principle which underlies and regulates this proceeding is that on which he lays some stress—viz., to restore, if possible, the bulbous urethra to its natural size, regarding it as about twice that of the external meatus.

"In order to effect this purpose," he continues, "I have designed and employed an instrument which somewhat resembles others that have gone before it, but which have been differently used; such, for example, as that referred to above, and also some employed for gradual or slow dilatation, applied at intervals of two or three days during a considerable period of time. The main and the important distinction consists in the method of employing it; in effecting the object by a single application only, instead of by numerous repetitions. My instrument, however, is much more powerful than its predecessors. It opens the bulbous urethra to more than double the size of the meatus, and acts mainly on the strictured, not on the healthy, portion of the urethra. When closed for introduction, it equals in size about a No. 5 catheter. As many strictures on which it is necessary to act are smaller in calibre than this, it is then essential to tie in a small gum catheter for thirty-six or forty-eight hours before employing it, in order to dilate the stricture temporarily to a sufficient size to admit the instrument. It is assumed, of course, that the stricture has been proved not to be permanently benefited by such (*i. e.*, tying-in) dilatation, or the treatment about to be described would not be adopted. However narrow or retractile a stricture may be, it is always possible, by tying in, to dilate temporarily as far as to the required calibre.

"The next principle which guides the treatment is this. Experience shows that most living tissues, if over-distended, lose to a great extent their natural elasticity. Thus the over-distension of the female urethra has often produced incurable incontinence arising from this cause—which is one instance only among others which might be adduced. The distending instrument I employ is composed of two parallel rods, which can be slowly separated by means of a screw in the handle, through the action of two levers which exist between them.* In this manner the rods pass gradually and successively through all the numbers of the catheter scale, from 5 to 16 or 18, in obedience to the operator's action on the screw, and an index there marks the rate of movement for his information. When opened, the two blades produce the figure of a very

* Originally one lever only existed. Recently Mr. Coxeter has constructed the instrument with two levers, and thus has augmented its power, which is now ample.

elongated spindle, the centre of which corresponds with the site of the stricture in the urethra. This position is easily ensured by means of the graduated scale marked on the stem of the instrument, and a small clamp which slides upon it. If the stricture is five inches from the orifice, this clamp is placed against the figure 5 on the stem, and the instrument is passed until the clamp rests against the external meatus. It is to be carefully maintained in this situation while the distension takes place.

“Having so placed the instrument, nothing would be easier than to turn the screw rapidly, and at once raise the calibre of the strictured portion to the required size—say 16 or 18. This would effect its rupture, and is precisely that which I wish to avoid. I occupy from seven to ten minutes in slowly distending the part up to that calibre, and therefore prefer to give the patient the benefit of chloroform. Hence I *over-distend as much as possible, and rupture as little as possible*. The tightest fibres of all alone get ruptured; those which are less rigid yield to the distending force. The more I can accomplish the latter object, the more I avail myself of the principles just alluded to, and deprive the tissues of their elasticity. The more I accomplish by rupture, the more of a wound I produce, and consequently the more fear there is of rapidly returning contraction. Having reached the limit intended, the screw is slowly turned back until the index marks about No. 10, and the instrument is withdrawn. The instrument is designedly not entirely closed. A large gum catheter is passed, and tied in for twenty-four hours, a plan which appears preferable, although there is no more occasion to do this than there is after rupture, under which circumstances Mr. Holt dispenses with the catheter altogether. I pass no instrument for two days; then a full-sized bougie every day or two, prolonging the intervals, and teaching the patient to do it himself occasionally afterwards.

“A few words on the cases in which this proceeding is not applicable, or rather in which other treatment appears to me preferable. For an old and non-dilatable stricture within two or three inches of the meatus, I prefer internal urethrotomy. It is perfectly safe and easy if performed in that situation. Dilatation and rupture, in my experience, are both inadequate to produce any lasting benefit. I have heard of my mode being tried at the anterior part of the canal. I should not have advised it. The spongy body here is so much less extensible, so little porous, and so greatly fibrous, as compared with its character where it constitutes the bulb, that but small room exists for extension. Hence in part the futility of ordinary dilatation as well as of over-distension in this situation.

“Again, if a urethra is narrowed at several points—an unusual condition, but nevertheless one which is occasionally met with—I should prefer to adopt Mr. Holt’s plan of rupture as, on the whole, the most certain to hit all the points of narrowing.

“There remain then all the strictures, forming a great majority, which exist at from four to six inches from the meatus. For these I may say, in one word, that, having employed all the mechanical methods of dealing with them that rest on sufficiently sound principles or on sufficiently good authority to merit a trial, I know of none that has afforded me such good results, both in regard of the immediate object and of the

enduring result, as the method I have endeavoured to delineate in this paper."

ART. 108.—*A New Remedy in Gonorrhœa.*

By J. S. PRETTYMAN, M.D., of Milford, Del., U.S.

(*Amer. Journ. of the Medical Sciences*, July, 1866.)

In July, 1859, while narrowly observing the effects of oil of erigeron administered in a fearful hæmoptysis, Dr. Prettyman was led to suspect that it would prove a useful remedy in the treatment of gonorrhœa. Acting upon this presumption, he immediately commenced giving it to a patient then under his care in whose case all the vaunted specifics had most signally failed. He improved at once, and was speedily cured. Since that date Dr. Prettyman has prescribed it in about fifty cases, with unvarying success. It arrests the discharge in about seventy-two hours, and effects a cure in from six to eight days. He does not recommend it as a specific in all cases, but designs merely to bring it to the notice of the profession as an exceedingly valuable medicine in this disease. Of course, all scientific medical practice is based upon the well-known pathological condition of the structures involved, and this is our unerring guide. When, in recent cases, the urethral inflammation is severe, his plan is to precede the remedy with a full dose of some active hydragogue. A good formula is: *R.*—Pulv. senna, ʒij; pulv. jalapa, ʒj; pulv. aromaticus, gr. x. *M.* Add a gill of boiling water and a teaspoonful of sugar, and, when sufficiently cool, agitate, and swallow at a dose. As soon as this operates, give ten drops of the oil on sugar, and three hours later a full dose of spts. æther. nit. in infus. althea, and so on every three hours alternately until the urethral irritation is allayed. Then leave off the latter, and continue the oil until the cure is complete. If the case is not recent, or there is but little urethral irritation, the oil alone is sufficient.

Dr. Prettyman has used it also in combination with copaiba and other articles, and found such preparations to answer a good purpose, but no better than the oil alone.

The oil which he uses is reputed to be that of the *Erigeron Canadense*; but he presumes that from the *Philadelphicum* is equal, if not superior, for this purpose.

ART. 109.—*On Soluble Bougies.*

By Mr. HENRY THOMPSON, Surgeon to University College Hospital.

(*The Lancet*, May 12, 1866.)

Believing that the imperfect action of injections depends upon the very short time that they are in contact with the mucous membrane,

Mr. Thompson conceived the idea of applying the astringent in such a form as would enable it to remain for a much longer period in contact with the inflamed surface. Under his direction, Messrs. Bell and Co. have constructed 'soluble bougies,' two or three inches in length, made of cacao butter, containing the drug it is wished to apply. They are cast in moulds, are perfectly firm and smooth, and may be used in any length, but that named has been deemed the best. A soluble bougie is equal in size to about No. 8 or 9 of the catheter scale, and may be introduced (having been previously oiled) by the patient himself into the urethra, where the material gradually melts in the space of about ten minutes. The patient is directed to slip one of these bougies into the passage on going to bed.

"After trying many methods for retaining the bougie *in situ*, Mr. Thompson has adopted the following:—A piece of adhesive plaster is cut, nearly an inch wide and five inches long. A piece of Taylor's stout lint, of the same size, is rolled up into a little pad and laid on the centre of the plaster, which is warmed, and applied along the lower surface and dorsum of the penis, the prepuce meanwhile being fully retracted. A second strip of plaster, half the width of the first, is then put closely around the glans penis transversely. The bougies are made to contain either a quarter of a grain of nitrate of silver, a grain of tannin, two-thirds of a grain of acetate of lead, or ten grains of nitrate of bismuth, as astringents; while others are sedative also, and contain two grains of opium, or two of belladonna. Other materials can, of course, be employed. By this plan Mr. Thompson has satisfied himself that the active agent is kept for several hours in contact with the urethral surface, and is, moreover, necessarily squeezed into the lacunæ, which often, doubtless, escape being acted upon by injections.

ART. 110.—*Insufflation of Medicated Powders into the Urethra.*

By M. MALLEZ.

(*Journal of Practical Medicine and Surgery*, May, 1866.)

M. Mallez has devised for this purpose an apparatus which consists—1, of an india-rubber ball supplied with a metallic mouth-piece; 2, of a slender catheter adjusted to a small brass cup, which can be adapted to the armature of the ball; 3, of a catheter of larger size, open at both ends. This sound is in the first place introduced beyond the membranous portion of the urethra: the narrow tube is then passed down into its cavity, bearing in its cup the medicated powder; and pressure of the india-rubber ball now affixed to the cup, propels the powder into the urethra, where it is deposited over the entire surface of the mucous membrane during the slow withdrawal of the apparatus. Experiments on the dead subject show that the operation perfectly answers the inventor's purpose.

In twelve cases of chronic gonorrhœa a cure was effected in this manner; and in five, the disease had lasted from two to four years.

M. Mallez has hitherto used bismuth powder. He is now engaged in experiments with other substances, such as the phosphate of magnesia. The instrument might be used for the introduction of medicated powders into the cervix uteri, and in almost every variety of sinus.

ART. 111.—*On the Operation of Trephining in Cases of Fracture of the Spine.*

By Dr. ROBERT M'DONNELL, F.R.S., Surgeon to Jarvis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University.

(*Dublin Quarterly Journal of Medical Science*, August, 1866.)

In a former article (see Vol. XLII. of the *Abstract*, p. 191,) Dr. M'Donnell discussed the general question as to whether the operation of trephining the spine should be adopted in certain cases, or whether it was to be set aside altogether as an operative procedure, as it practically has been for many years past. In the present paper he considers the special circumstances and symptoms which appear to indicate cases favourable for the operation, or the reverse; and finally the method of performing the operation in different regions of the spine.

He cites first, in greater or less detail, and with a view to the removal of certain objections to the operation, twenty-six cases of fracture of the spine, and adds the following remarks:—

“These cases show, if nothing else, at least that instances of fracture of the vertebræ, unaccompanied by fracture or displacement of the body, are not very unfrequent; in fact, I candidly confess, that until I had looked into the published details more carefully than I had hitherto thought of doing, I accepted the assertions of others, and was not prepared to find so many. Those which I have here brought together represent rather more than ten per cent. of the recorded cases upon which I have been able to lay my hands. In the great majority of such cases the fracture occurs in the cervical or upper part of the dorsal region; the reason of which is obvious, both from the conformation of the spine and the exposure of these regions to injuries by direct violence. In discussing the question as to whether the operation of trephining the spine is to be admitted among the legitimate and recognised operations in surgery, injuries of the kind above mentioned have a considerable importance. They prove that after all, among the cases of fracture of the spinal column, a goodly per-centage are of such a nature that even the sternest adversaries of the operation would admit them not to be unsuitable for it. The question here only turns upon the difficulty of diagnosis.

“The real question as to the fitness or unfitness of any case for operation will, I conceive, depend far more upon the exact amount of injury done to the spinal cord, than on whether the fracture engages the portions of the vertebra in front of it or behind. No doubt the operation would have a good chance of being brilliantly successful in

such cases as Case VII., XI., XIX., or XX.; but assuredly there are not a few cases in which, although pressure may arise from injury done to the vertebra anteriorly, yet removal of the counter-pressure posteriorly will set the cord free from being squeezed. When the body of a vertebra is broken, and, as usually happens, the intervertebral substance is at the same time more or less lacerated, the progress of the case must be, under the most favourable conceivable circumstances, very tedious. The first thing that nature does is to absorb the lacerated fibro-cartilage, and while this is being accomplished little or no callus is thrown out. I find, however, a considerable number of cases reported in which the body of a vertebra is so injured as to press upon the marrow, although no separation of it into fragments has taken place; neither are the fibro-cartilages torn."

"In injuries of this kind if, by taking away the posterior arch or arches, the pressure on the cord is relieved so as to enable the patient to escape from the immediate risks to life, there may be reason to hope that the patient may ultimately recover, as do cases of curvature of the spine from caries, where the deformity is as great or even greater.

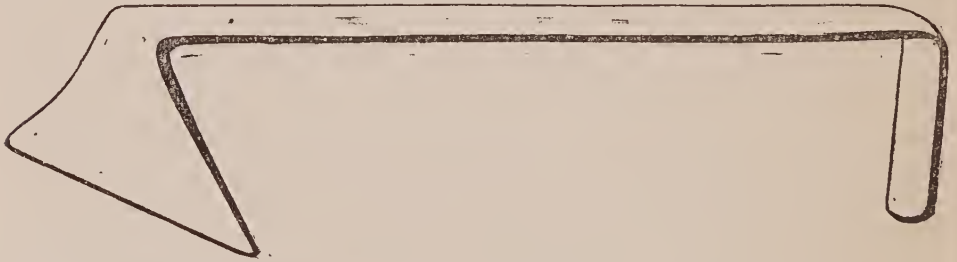
"When, in consequence of a fracture of the spine, the marrow is subject to pressure—but not lacerated—it is usual, unless the pressure be so considerable as to destroy both sensibility and motion below the injury, to find that the functions relating to motion are much more affected than those of sensation; the patient cannot attempt to move his toes or feet; cannot draw up his legs in bed, or even push them down if flexed for him, while feeling may be normal even to the tips of the toes; the sphincters may be paralysed, yet the patient distinguishes the temperature of an enema, and complains on the introduction of a catheter; if the spinal marrow be lacerated to any considerable extent, or what is equivalent to laceration, if it has been so roughly compressed as to disintegrate its structure, the case is different—sensation and motor power are then alike lost.

"Between these two supposed conditions there are many degrees; and an accurate examination of the symptoms connected with sensation must be the chief aid in making an accurate diagnosis. The amount of sensibility on one side as compared with the other, and estimated by application of the points of a pair of compasses—the appreciation of the sensation of heat and of cold—of contact, of tickling, and of the position of the limbs—should each be looked to. The amount of voluntary power affecting certain muscles will assist in determining the seat of pressure; and if paralysis of motion is found on one side in a degree much more marked than on the other, while sensibility is observed to be greatest in the part where motor power is least, there need no longer be any doubt that the pressure is chiefly exercised on the lateral half of the cord on that side of the body on which the motor paralysis is found to be greatest."

Dr. M'Donnell next discusses somewhat in detail the value of different symptoms in aiding the judgment, and he here proceeds to describe the operation, premising that there is nothing to contra-indicate the free use of chloroform. "The mode of performance, and difficulties encountered in accomplishing the operation, vary much in

the different regions of the spine, and, of course, in actual practice must vary with the nature of the case and the mode of injury. When the spinous process and laminae only are broken the whole proceeding will be very simple; but even when this is not the case it may be said, that compared with many of the great operations in surgery, that of trephining the spine is not so formidable as has been supposed.

“In the lumbar region and lowest part of the dorsal the operation offers the greatest difficulties. The patient should be placed face downwards, with the feet towards the window, so that the light may fall into the bottom of the wound. The body should be placed as evenly and flat as possible on its front aspect; but the person who gives chloroform can keep the head turned a little on one side, and somewhat raised, by placing one of his hands under the chin. An incision from four to five, or six, inches long is to be made through the integuments over the spines, with a strong scalpel. A strong curved bistoury is the best instrument with which to divide the muscles on each side of the spinous processes; and this can be done at once more rapidly, and closer to the bone, by entering the point of the bistoury below and cutting upwards and backwards. As soon as the mass of muscles on each side are to a sufficient extent divided, the operator should fix, himself, on each side, a retractor such as is here represented, and give each in charge to an



assistant. The broad flange of such a retractor not only keeps the wound well open, so that the operator can see what he is about, but by pressure prevents hæmorrhage, while the hand of the assistant who holds it does not get fatigued, owing to the other end being bent. The operator should next feel in the sulcus, on each side of the spinous process, for the inequality caused by the injury, and by grasping successively each spinous process, in a pair of necrosis forceps, ascertain whether the posterior portions of any vertebra are broken. He must then determine which laminae are first to be divided; and before doing this it is well to place sponges, wet with iced water, in the hollows on each side of the spinous processes, and give time for all hæmorrhage to cease.

“Having singled out the spinous process of the vertebra, the laminae of which are to be divided, the interspinous ligaments above and below it are to be cut, and a part of the process taken off with bone forceps, leaving, however, enough to grasp with the necrosis forceps, when it becomes necessary to remove that portion, after division of the laminae. The division of the laminae, in this region, is exceedingly difficult. The trephine cannot be used; and Hey’s saw, owing to the depth of the wound, and oblique manner in which the bone must be sawn, is also useless. The division is best accomplished by strong forceps bent at an

angle, something like the forceps known as "Fergusson's side-cutting forceps." Experience shows that the laminae may be thus cut through without fear of injuring the cord or its membranes. As soon as this has been accomplished, the portion of the spinous process still remaining is to be caught in a pair of necrosis forceps, and the portion of bone raised and removed by carefully dividing the ligamentous structures still holding it. When the posterior arch of one vertebra has been taken away, the removal of the second is comparatively easy; the best method of effecting this is by nibbling it away bit by bit, with the ordinary gouge forceps.

"It is a much easier matter to remove a spinous process, and the laminae on each side of it, in the middle or upper part of the dorsal region than in the loins. In its preliminary stages the operation is like the foregoing. When the muscles on each have been divided and retracted, as already described, owing to the mode in which the spinous processes of the dorsal vertebrae are imbricated one over the other, it becomes necessary to remove the spine above that which, with its laminae, is to be first taken away. This having been done, the spinous process of the vertebra, the laminae of which are to be divided, should be taken away, as close to its root as possible, with gouge forceps. With a little care the spinous process can be so completely taken off as to leave the surface sufficiently flat for the application of the crown of a trephine, which should in general be about one inch in diameter, and furnished with a strong centre pin, a little longer than usual. As the bone is of a much softer texture than that of the cranium, the trephine once planted, works rapidly, so that after a few turns the centre pin should be withdrawn, and it should then be worked gently until it is found possible to move, and subsequently elevate, the trephined portion; after this is taken out it will be seen that in the dorsal region there is a sort of provision which enables one to use the trephine without much danger of its, in any way, injuring the cord. In fact, the articulating processes of the vertebra, next below, look backwards in such a way as to prevent the instrument from going suddenly in so as to injure the medulla. The posterior arch of a second vertebra may now be easily taken away, either by a second application of the trephine, or better still, by the aid, simply, of the gouge forceps.

"In the cervical region the removal of the bone can be effected with great facility by the gouge forceps alone; here, as well as in operating on the first dorsal vertebra, the trephine cannot safely be employed. After the skin has been divided and the muscles detached on each side the interspinous ligaments are to be cautiously cut, taking care not to sink the scalpel too deeply; the spinous process is next taken off with the gouge forceps, and at once the ligamentum subflavum comes into view, and the vertebral canal is opened to a small extent. Bit by bit the posterior arch is then to be taken away by the gouge forceps, until enough is removed. The removal of a second or third can be even more readily accomplished with the same instrument. The retractors employed for holding back the muscles on the neck should not be so broad as those intended for the operation on the back or loins, and the lower edge of the retracting flange should be somewhat convex.

"In all the cases which I have seen the wounds have gone on

favourably under the simplest treatment; water dressing at first; and after suppuration has become established, charpie, with simple dressing, or a poultice with some "warm dressing" under it.

"It is unnecessary to say that every possible attention must be paid to the general health of the patient. He should be placed on a water bed, the bowels and bladder carefully attended to, and any tendency to bed-sores guarded against with the utmost anxiety. For this purpose he must be shifted from side to side, and occasionally placed upon his back, even at the risk of pain in the wound or disturbance of the broken vertebra. To avoid bed-sores and keep the bladder in good order must be the first object of the surgeon, and no entreaties or complaints should influence him in deferring to do what he considers necessary for this purpose. The bed clothes must be frequently changed, and kept perfectly dry. If any bed-sores have already formed, every effort should be made to get them cured; if the urine is already alkaline and mucopurulent the bladder should be washed out every day, or several times each day.

"Immediately after the operation the object of medical treatment should be to avoid inflammation of the cord or its membranes, and with this view belladonna or atropia should be administered, with the intention of keeping the medulla as much as possible in a condition of repose; opium should not be given; but if urgently demanded to allay pain, it should be joined with belladonna, in doses proportionally large. Somewhat later iodide of potassium may be ordered, and when all apprehension of inflammation is past, strychnine. Indeed, it will be in different cases a very nice and at the same time very important question to determine whether it may not be good practice to give strychnine early, even at the risk of calling forth inflammatory action in the cord. Of course, its administration would not be contemplated if any tendency to myelitis can be detected. But if pressure on the medulla for a longer or shorter time has given rise to that kind of mechanical softening of it which without being identical with white or atrophic softening, is closely akin to it, and if it seems that the parts below are not recovering their vital powers after the pressure has been got rid of, it may become necessary to make a desperate effort to rouse the dormant energies of this portion of the marrow, regarding the chance of exciting inflammation of it as the less great danger of the two. Under these circumstances I should not hesitate to give strychnine cautiously before many days after the operation.

"Shampooing of the limbs and the use of electricity should be early applied, in order to arrest the atrophy of the muscles. Whatever doubt may exist as to giving strychnine at an early period after the operation there need be none about its administration after the risk of inflammation is past; it should then be added to good nourishing diet, with wine, beer, and iron tonics."

(C) CONCERNING THE UPPER EXTREMITY.

ART. 112.—*Excision of nearly the whole of the Ulna.
Reproduction of the Bone.*

By J. K. WEIST, M.D.

(Cincinnati Lancet and Observer, March, 1866 ; New York Medical Journal, April, 1866.)

Col. C. received, Oct. 27th, 1864, a gunshot wound in the left arm. The ball entering on the inside of the arm about two inches above the wrist joint and passing upwards, made its exit on the outside of the arm about three inches below the point of the olecranon, badly breaking up the ulna in its passage, leaving, however, both of the articulations intact. Two days after the receipt of the injury it was deemed advisable to remove the broken fragments of the ulna by an operation. This was done by making an incision down to the bone, and extending from the wound of entrance to that of exit. Many fragments of bone were removed, which together made up almost the entire bone. There only being left behind about two and a half inches of the upper and one inch of the lower end, the broken ends of these portions of the bone left behind were smoothly sawn off. Although the periosteum in this case was much lacerated and torn, it was carefully separated from each fragment removed, and its connexions as little disturbed as possible. After the operation the arm was placed upon a splint, and the wound treated in the usual manner. And although this officer was unavoidably placed under bad hygienic influence in general hospital, where considerable sloughing and secondary hæmorrhage occurred, greatly protracting the cure, the final result was highly satisfactory, as the lost bone was reproduced, and when Dr. Weist examined the arm last October, just one year after the operation, all of its functions had been regained, with the exception of a slight loss of power to rotate the arm. The new ulna, though not quite so symmetrical, seemed to have all of the solidity and value of the one in the uninjured arm.

ART. 113.—*Successful Extirpation of the Entire Left Scapula
and Acromial End of the Clavicle, with Preservation of
the Arm.*

By A. HAMMER, M.D.

(St. Louis Medical Reporter, March, 1866 ; New York Medical Journal, May, 1866.)

The case was that of a young lady, about eighteen years of age, having a tumour situated upon the exterior surface of the left scapula. The size of the tumour was about that of an orange, occupying the infra-

spinous fossa, scarcely moveable, of a round shape, a smooth and even surface, and the skin of natural colour, not adhering to the tumour, but free and moveable upon it. Palpation conveyed the idea that it was a well developed fibroid. No pain was felt upon the most severe pressure, and only to a slight extent in the afflicted region when the arm was made subject to sudden and forcible action. No swelling of either the axillary or supra-clavicular glands; and all the functions of the general system were, in fact, perfectly normal. Her whole appearance was the personification of general good health. The history of the case is as follows:—About two years ago she unexpectedly discovered a small tumour, the size of a hazelnut, the one in question, and situated as already described. Its presence occasioned no inconvenience, much less any pain. From that time on it grew slowly and steadily until it reached the size mentioned. Since early infancy she had never been subject to any sickness, and, within her recollection, had never received a mechanical injury of the afflicted scapula. Dr. Hammer concluded that the tumour was a hard, fibrous growth, having a broad base, and arising from the periosteum of the scapula. He, therefore, proposed the extirpation of the tumour. In the latter part of September, 1860, he proceeded to the operation. Under the full influence of chloroform, an incision was made a little above, and parallel with, the spine of the scapula, its entire length, and then another parallel with, and about an inch to, the inner side of the posterior or vertebral border of the scapula, extending to the inferior angle. These incisions admitted of two flaps—one, the larger, and situated external to the vertical incision; the other, internal. After the dissection, Dr. Hammer at once discovered his mistake of diagnosis; the hardness of the tumour, wherein it simulated fibrous tissue, was gone. It was now soft, elastic and easily compressible, adhering firmly to the bone, and slightly encroaching in some situations upon its margins. He remarked that the tumour was most likely of a malignant character, either a sarcoma or true cancer, which would require resection of the scapula, either in part or *in toto*, according to the extent of the growth. The operation was, therefore, discontinued. The microscopical examination confirmed his supposition, exhibiting the elements of the encephaloid form of cancer. Being now intimately acquainted with the nature of the disease, Dr. Hammer proposed the extirpation of the entire scapula as the only means of removing the tumour. The margins of the incisions had become thickened and infiltrated with cancerous elements to the width of about half an inch. These were freshened by making new incisions at least one inch distant from the former ones, extending the first incision parallel with the spine of the scapula upwards and forwards upon the acromial end of the clavicle. The insertion of the trapezius and deltoid to the spine of the scapula was cut away, as were also the attachments of muscles to the vertebral and superior borders; then about three-fourths of an inch of the acromial end of the clavicle was removed by the chain saw, the shoulder joint opened by a transverse incision, and the head of the humerus dislocated. The origin of the short head of the biceps, coraco-brachialis and pectoralis minor was detached from the coracoid process, the subscapularis cut away near its insertion into the lesser tuberosity, and the exsection completed by dividing from above

downwards the supra-spinatus, infra-spinatus, and the muscles arising from the axillary border; the latissimus dorsi was saved by separating it from the teres major at the interior angle. After the hæmorrhage had been arrested, the wound was united by eighteen wire sutures, the arm then brought into such a position that the head of the humerus exactly corresponded in its relations to that of the opposite side, supported by a wedge-shaped pad placed in the axilla, and retained by a bandage similar to that recommended by Desault for fracture of the clavicle. The wound healed by first intention in by far its greater extent. The resected end of the clavicle effected a circumscribed inflammation, which resulted in an ulceration of the skin and exposure of the bone; the cut surface, however, became necrosed in the space of three weeks, and was detached. Healthy granulations then sprung up, followed by rapid cicatrization—the cicatrix, of small size, firm, and somewhat funnel-shaped, connecting securely the clavicle to the skin above and the rib beneath. In six weeks the wound had entirely healed and she was perfectly well; yet it was necessary to support the arm in a handkerchief fastened about the neck. In another six weeks, three months after the operation, the arm was secured in its new position, the head of the humerus being firmly fastened to the ribs by fibrous adhesions and the formation of a sort of new glenoid cavity. In March, 1861, the disease recurred in the shape of a small glandular swelling in the supra-clavicular region. It increased steadily and rapidly, until it reached the size of an egg, when it was removed. In a short time it reappeared in the cervical vertebræ, and rapidly invading the spinal marrow, the patient died in July, 1861.

ART. 114.—*Successful Resection of both Bones of the Forearm, for Ununited Fracture of long standing.*

By SAMUEL J. JONES, A.M., M.D., Surgeon U.S. Navy.

(*American Journal of the Medical Sciences*, July, 1866.)

Charles Donald, aged forty-three, mariner, constitution good, received a fracture of the radius and ulna of his left forearm, produced by the fall of a spar in New Orleans, in March, 1864. The fracture was dressed at that time, but bony union did not follow. After the accident the mobility continued to be so great at the seat of injury—the junction of the middle and upper thirds of the forearm—that an effort to hold the forearm in a horizontal position was attended with marked inclination of the limb below the seat of injury. Every attempt to use the limb, caused it to become swollen and painful, thus preventing him from engaging in any employment.

Fifteen months after the original injury, Dr. Jones first saw the patient, and found the condition described, and at least one inch of shortening in the forearm. By making an H incision on the dorsal aspect of the forearm, one incision being made lengthwise over the radius, and another over the ulna, and uniting the two by a transverse

one made at the points of the fracture, the parts were exposed, showing that firm ligamentous union had taken place, binding down the ends of the bones. The fragments had been prevented from uniting by the interposition of muscular fibres, around which the ligamentous bands had accommodated themselves. The fracture was an oblique one, and the lower fragments were over-riding the upper, thereby causing the shortening. The ligamentous bands were divided in the operation, and the fragments that had become rounded at the ends were removed by means of a chain saw, so as to bring the square surfaces in apposition, which left the arm still about one inch shorter.

The hæmorrhage was very slight, and the wound was closed with interrupted stitches, and an internal and an external splint applied; the latter having a rectangular opening left at the point of incision to dress the wound. Cold water-dressing was applied.

It soon became apparent that those splints did not accomplish the desired object of keeping the ends of the bones in apposition, and Passed Assistant Surgeon William C. Lyman, U. S. Navy, suggested the use of a modification of the ordinary fracture-box, in which the forearm should be supported by means of adhesive plaster secured to the outside of the box, and descending two-thirds of the distance to the bottom on the inside, and both bones kept in a vertical position, by packing each side of the forearm with charpie. This packing could readily be graduated as occasion demanded, and the adhesive-strips afforded sufficient support for whatever pressure was made, and the temperature was easily regulated by moistening the charpie with water at the temperature desired. The limb did not reach to the bottom of the box by an inch or more, and the dripping resulting from any excess of discharge or too free use of the water-dressing, was prevented from running out of the box into the patient's bed, by means of pieces of lint or other absorbing substance placed in the bottom of the box, and readily removed, whereby the dressing was rendered very clean and neat, as well as very cool. Two difficulties were, however, encountered. The strip of adhesive-plaster at the point where the ends of the bones were in apposition, seemed to press too directly on them, making an acute angle in the ulna, instead of allowing it to lie horizontally. This was remedied by placing a splint on the strips, on which the forearm rested throughout its length.

The second difficulty was to give to the box sufficient motion to prevent movements of the body from displacing the fragments, if those movements were sudden, as was the case while the box lay upon the bed by the side of the patient. This was remedied by suspending the box from a point higher than the patient, who raised or lowered the box himself at will. Thus, after a few weeks, he could rise from his bed, and occupy a chair by his bedside, by simply shifting the suspending cord to the desired height.

The arm was kept in the box for eight weeks, and on its removal therefrom it was found that the union was sufficiently firm to prevent dropping of the hand when the arm was held in a horizontal position as it had done before, but the union was again only ligamentous.

After allowing about a month to elapse for the patient to recover from his confinement, the original incisions were reopened, and the

ligamentous bands with less than one-fourth of an inch from each fragment of the bones removed, again bringing the flat surfaces in apposition, and firmly securing them there by means of silver wire. The forearm was then again placed in the fracture-box, and treated as in the first operation. At the end of eight weeks it was removed from the box, and supported for a short time by a temporary splint, the incisions having closed except where the twisted wires were projecting.

On Dr. Jones's detachment from duty at New Orleans, the case was left in charge of Dr. Heber Smith of that city, who subsequently removed the wires by untwisting them and forcibly drawing them out. He reports that in December, six months after the first operation, and twenty-one months after the original injury, the openings had finally closed, and osseous union had taken place in both radius and ulna, giving the man a quite useful arm, though nearly one and a half inch shorter than the other. The displacement of the fragments from the time the injury was received caused the muscles of the forearm to adapt themselves to the change, and the absorption caused thereby left the muscles in proportion to the length of the bones remaining, which proved to be an advantage.

The dressing used in this case commends itself for its cleanliness, for the ease with which it can be applied and changed, and for the convenience with which pressure can be graduated as desired. It seems applicable to all cases in which the fracture-box is ordinarily used.

During the eight weeks that the limb was each time in the box, it was only necessary to change the adhesive strips once, except the one strip at the point of the transverse incision where the discharge was greatest, all the strips adhering firmly to the wood until removed.

ART. 115.—*Case of Fracture of both Clavicles.*

By M. FOUCHER.

(*Journal of Practical Medicine and Surgery*, 1866.)

The following case of this rare accident was admitted into the Hospital St. Antoine, under the care of M. Foucher. The patient was a stonemason, aged thirty-eight years, and he had been injured by the falling in of a ceiling. When he was brought into the ward, on the 7th May, he was in a comatose condition, his breathing was stertorous, and he appeared to suffer pain on pressure of the thorax. On the 8th, M. Foucher found him conscious; he complained of difficulty of breathing, but had not expectorated any blood. Both clavicles were fractured in the middle. On the right side the fragments were not much displaced or moveable, but on the left the fragments overlapped each other, and their position could not be corrected. The ribs were apparently uninjured, but the right fibula was broken. When the ceiling fell in the patient was working in a crouching position, and had fallen backwards. Two triangular slings were applied for the support of the elbows, and

the man was ordered to lie flat on his back. Fortunately for the cure of the fractures of the clavicles, the patient was compelled to keep his bed up to the 1st of June, on account of an eschar caused on the right leg by the pressure of a bandage prepared with the silicate of soda, despite a thick padding interposed between the apparatus and the integument.

On the 12th of June he was removed to the convalescent hospital of Vincennes. The collar-bones were scarcely deformed, although on the left side the callus was irregular and voluminous. The movements of the arms were free, and painful only when any attempt was made to raise a weight.

This case is a fresh illustration of the inutility of bandages in fractures of the clavicle, provided the patient keeps the horizontal attitude. This view has repeatedly been urged by Mr. Vincent; and we may further observe that fracture of both clavicles is so unfrequent that Malgaigne adduces but six instances of the double injury. This author makes no mention of the great difficulty of breathing noticed in the present case, and he adds that in his patients consolidation was imperfect, and even failed altogether in three of the subjects. In the instance we have recorded above, consolidation was effected under the unaided influence of the horizontal attitude, and it may safely be asserted that the subsequent deformity will be extremely unimportant.

ART. 116.—*On Dislocation of the Shoulder Joint.*

By Dr. G. HAMILTON, Falkirk.

(*Edinburgh Medical Journal*, September, 1866.)

Dr. Hamilton offers some suggestions on the reduction of a dislocated shoulder-joint, first quoting the opinions (not always favourable) of those surgical authorities who have discussed the method of procedure, a modification of which he advocates. He says:—

“About two years since, I met with rather a difficult case, in the person of a large-bodied and very muscular man, in which I took advantage of a huge arm-chair, with a strong high back, which I found in the house. On this I placed a pillow, for the axilla to rest upon, and with the assistance of two strong men I reduced the dislocation very satisfactorily. Another followed, shortly afterwards, where I had no suitable arm-chair, but where I found a common screen for drying clothes, and this, with the pillow, also did very well. In a third case, neither of these being at hand, I mounted the patient on a table, placed the axilla on a pillow on the top of a door, and succeeded equally well. About six months since, I had, unfortunately, to make personal acquaintance with this accident. In passing over a railway bridge, my horse took fright at a passing train, and came down with me. In stretching out my right arm to save myself, dislocation at the shoulder took place, of which I was immediately made aware by the ugly tearing sensation that occurred. Fortunately, a house was near at hand, in which I

received shelter. Without losing a moment, I looked about for some suitable apparatus with which to effect reduction. Finding nothing better, I got a narrow table, on which I placed, on its side, a long narrow stool, such as is found in cottars' houses. On the top of this I had a pillow placed, on which I rested my axilla, my body being placed between the two feet of the stool. Two strong men, who were at hand, kindly lending their assistance, reduction was effected after a few minutes' traction. I was so much pleased with the results in these instances, that I was thinking of having constructed a suitable apparatus which I could keep by me for use in such dislocations, when I cast my eyes upon a set of painters' steps, which immediately struck me as precisely the article I wanted. I have used this now in three cases, and its use seems to me to give very considerable advantages over the modes of reduction generally employed.

"The 'steps' I use are 4 feet 10 inches high, and the moveable support should be fixed with an iron rod, and not with a rope, as is often the case, as the former secures a greater amount of steadiness. A pillow is laid across the top step, and the patient ascends as high as may be convenient, of course placing the axilla on the top of the pillow. One or two assistants now lay hold of the arm, drawing, at first, steadily outwards and slightly downwards, traction in the latter direction being gradually and cautiously increased by approximating the arm to the steps. Reduction, in all the cases I have had, has been effected easily, and even, if I may use the expression, elegantly, but none of the dislocations had remained unreduced for more than twenty-four hours. The great power that we here possess, however, seems to me to render it highly probable that, in cases of longer standing, this simple apparatus will also be found very efficacious.

"The three agencies mainly to be relied on in ordinary cases of shoulder-joint dislocation are evidently extension, counter-extension, and leverage, and especially the combination of these. When the dislocation has remained long enough unreduced for adhesions to form, perhaps, also, the putting in practice preliminarily some such manœuvre as Sir Astley Cooper saw the Lancashire bone-setters use, where they rapidly whirled round the arm before attempting reduction, may be of importance to the operator.

"In using the 'steps,' their height is very convenient for exercising extension, while the counter-extension required is made to a great extent by the weight of the patient's body, the rest being easily supplied by the foot of an assistant. The height, again, is very important in exercising leverage power, and its amount at command is enormous, and of course requires caution in its use. In laying hold of the arm of a person placed in position for experiment, I have the feeling that I could with ease, if I wished, produce either dislocation or fracture of the humerus. Here, also, the combination of these powers is easy and natural, simply by causing the assistants to approximate the arm to the steps. Almost all our best surgeons have dwelt upon the importance of employing leverage in these cases, and yet the usual modes of reduction supply this very inefficiently. The heel in the axilla, or the knee of an assistant, gives us but little; while, when the pulley is employed, leverage power, from the points of extension and counter-extension being

fixed, is necessarily lost altogether. To remedy this, I recollect seeing Mr. Liston, as he recommends in his *Operative Surgery*, endeavour, with a towel under the patient's arm, to *lift up* the head of the humerus; but the power given by this means is evidently very slight compared with such leverage as can be got in using the 'steps.' With these, even should the pulley be used, leverage could easily be combined with extension, by gently moving the steps forwards; or, perhaps, this might be done more effectually and continuously by having wheels attached to the steps.

"In brief, this modification of the usual modes of reduction of these dislocations, which I have proposed, seems to possess the advantages:—

"1st. Of enabling the surgeon to dispense with his personal exertions.

"2nd. It gives an amount of power in extension and leverage limited only by a consideration of the resistance possessed by the tissues; and it also enables the operator easily and naturally to combine these powers.

"3rd. The position of the patient gives perfect freedom for the administration of anæsthetics, if such should be wished or required."

(D) CONCERNING THE LOWER EXTREMITY.

ART. 117.—*Dislocation of the Head of the Fibula, with Fracture of the Tibia.*

By Dr. FOUCHER.

(*Gazette des Hôpitaux*, 1866; *Schmidt's Jahrbücher*, 1866.)

A man, thirty-six years old, going home at night tipsy, quarrelled with a passer-by and received several kicks on the right leg, from the effects of which he fell and could not get up again. He was conveyed to the hospital of St. Antoine, and when examined on the following day, it was found that the right tibia was fractured at the junction of the upper with the middle third. The fracture was oblique, and the upper fragment projected forwards. Notwithstanding some swelling, an unnatural projection was observed at the upper part of the leg on the level of the spine of the tibia, and close to the insertion of the ligamentum patellæ. Behind this projection was a cord-like elevation. The projection was evidently the displaced head of the fibula, and behind it could be felt the articular facet. The fibula was unbroken, and held an oblique direction from above downwards and backwards. The knee was somewhat flexed, and the tendon of the external head of the biceps formed the cord-like elevation. The foot was somewhat abducted, the peronei muscles tense, and the patient had not only acute shooting pains in the leg, but also sensations of numbness and tingling. The displaced head of the fibula could be easily reduced without noise, and its mobility was so great that it could be carried back behind its proper position. The fibula could indeed be moved as a whole, and could be lifted off from the tibia at both its extremities; so that the ligaments

binding it to the astragalus and calcaneus must have been torn. The ankle-joint appeared wider than usual between the malleoli. The fracture being reduced, and the fibula replaced, the leg was somewhat flexed, and laid in a padded fracture-box. In six days the swelling had subsided, the pain from the injury to the peroneal nerve had ceased, and the head of the fibula was stationary in its proper position. A starch bandage was then applied, and worn until recovery was complete.

Dislocations of the upper tibio-fibular articulation are so uncommon, that every recorded example possesses interest for surgeons. In this case the rapid recovery is remarkable.

ART. 118.—*Case of Dislocation of the Foot forwards.*

By Dr. WILLEMIN.

(*L'Union Médicale*, 1866 ; *Schmidt's Jahrbücher*, 1866.)

A woman, thirty-eight years old, of lymphatic temperament, but in good health, slipped upon the floor of a room while carrying a heavy child in her arms. She fell backwards, with her right foot bent under her, so that the heel struck against the nates. She immediately felt acute pain in the right foot, which increased, and produced faintness. When examined, soon after the accident, the foot was in dorsal flexion, and presented on its summit a projection two or three centimetres in height, that could be identified through the tense integument as the articular surface of the astragalus. The malleoli were uninjured, and the projection of the calcis was displaced forwards to an extent corresponding with the elongation of the dorsum of the foot. The foot was quite immovable by the patient. Grasping and elevating it, the author brought it gradually into plantar flexion, and pressed the leg forward with the other hand. After a few minutes reduction took place with a loud snap. Wet compresses were applied round the ankle ; and when, in a few hours, considerable painful swelling followed, bladders of ice were substituted for them. Much ecchymosis appeared, the swelling remained the same for six days ; the patient complained of pain from the slightest touch, and the skin about the ankle was very sensitive. After the sixth day warm compresses with lead lotion, and after the tenth day simple wet compresses were applied. By the seventeenth day the swelling had much subsided, and a brownish-red discoloration extended from mid leg to ankle. The ankle-joint itself was still somewhat swollen and sensitive. A starch bandage was applied, and left undisturbed for fourteen days, by which time the swelling had almost disappeared. While renewing the bandage, the author felt a crepitant friction in the joint, but could not discover any fracture. The foot possessed slight power of flexion and extension, but the lateral and rotation movements were more free than in the normal state, and during them the crepitus was felt. The points of the malleoli were further apart by six millimetres than in the uninjured foot, and the external malleolus seemed to possess an unnatural degree of mobility,

Professor Bach was called in consultation, and believed that the tibia and fibula had been separated. A starch bandage was worn for many weeks, and the crepitation disappeared. The patient was allowed to move about on crutches. On the fiftieth day a slight bandage only was applied, and a padded shoe. Unfortunately the swelling returned in great severity, so that the starch was again required, and ten months elapsed before recovery was complete.

The above form of dislocation is extremely rare, and only three or four instances of it have been recorded.

ART. 119.—*Elephantiasis Arabum, or Elephas, Successfully Treated by the Application of a Ligature to Main Artery of Limb.*

By THOMAS BRYANT, F.R.C.S.

(*Proceedings of Royal Medico-Chirurgical Society*, 1866 ; *British Medical Journal*, September 8, 1866.)

The author, having made some general remarks on the disease, related a case of elephas, which occurred in the person of Mary T., aged twenty-five, the daughter of Welsh parents, who was admitted into Guy's Hospital under his care on October 10th, 1865. She was a single woman, of healthy aspect, and had always enjoyed good health, never having had any illness of much importance. Ten years previously she had scarlet fever, which was unaccompanied by any of its ordinary complications; and it was during her convalescence from this disease that her left leg began to swell, the swelling beginning in the calf and extending upwards towards the knee; it was unattended by pain or any indication of general disease. For two years the enlargement was gradual, when she slept in a damp bed, and after this the disease progressed more rapidly, and extended upwards to the thigh. This increase was still, however, perfectly painless. She was subsequently admitted into the Carmarthen and Swansea Infirmary, where all kinds of treatment were tried, but without success, the disease gradually progressing. Three years since, some small ulcers appeared in a deep sulcus in the calf, from which a quantity of dark fluid like blood escaped; the ulcers subsequently healed.

On admission, the left leg was found to be enormously enlarged from the ankle to the groin. To the hand it felt hard and brawny, the skin and cellular tissue being evidently infiltrated with a fibrinous material. Several deep sulci also existed between the folds of integument in the calf. The skin appeared to be coarse, but it was free from the cuticular induration and ulceration which is so frequently associated with this affection. It was also noted that the foot was perfectly sound. The measurements on admission were as follows:—Round the left or diseased calf, 24 inches; round the right, 15½ inches; round the diseased thigh, 28 inches; round the right 21 inches. The temperature of both limbs appeared to be alike. The pulsation in the left iliac artery was

clearly to be felt; but the femoral and tibial vessels of the affected limb could not be made out. The patient was kept in bed for three weeks, with the leg well raised on an inclined plane. In the first week the calf had diminished an inch and a half, and the thigh one inch, all œdema having subsided; but after that date no further decrease took place. On the 31st of October the external iliac artery was ligatured, the patient being under the influence of chloroform. The vessel appeared to be perfectly healthy, and of normal size. The whole limb was subsequently swathed in cotton-wool, and raised as before. The subsequent progress of the case was one of uninterrupted success. The limb rapidly became softer and smaller, the calf measuring at the end of the week $19\frac{1}{2}$ inches, and the thigh 24 inches, being three inches less than it was on the day of operation. At the end of the second week the limb had diminished another inch; and on the fifteenth day the ligature came away from the iliac artery, the limb all this time having been free from pain, and quite warm. By the 30th of November the wound had quite healed, and the patient's health was very good. By the 30th of December the calf had become reduced to $18\frac{1}{2}$ inches; by January 31st it measured $16\frac{3}{4}$ inches; by February 21st it was only $15\frac{7}{8}$ inches; and on March 15th it measured $15\frac{1}{2}$ inches, being but three-quarters of an inch larger than the sound limb. The skin had gradually contracted, and had become natural in its aspect; all brawniness of the limb had also gone. The patient at the present date is walking about with an elastic legging, perfectly sound.

ART. 120.—*Case of Fracture of the Leg by Muscular Action.*

By Dr. HEVLHARD D'ARCY.

(*Journal of Practical Medicine and Surgery*, September, 1866.)

Dr. D'Arcy relates the following case:—

Mr. X., aged forty-two, the head clerk in a lawyer's office, a man of vigorous constitution, was standing, one evening in the course of last January, near a window, his hand resting on a sill. In moving away, his left foot slipped on the waxed floor; he made a violent effort to recover his balance and experienced a sudden pain, accompanied by a snapping sound in the right leg. He fell into a chair, from which he found it impossible to rise. Dr. D'Arcy was immediately summoned, and he discovered a fracture of both bones of the leg, somewhat below the middle region of the limb. The fracture of the tibia was extremely oblique, the sharp point of the superior fragment protruding beneath the skin, which fortunately was not perforated. The leg was placed in semi-flexion on a raised cushion so as to relax the extensors of the knee, the eighteen-tailed bandage was applied, and the fracture was closely watched. After an interval of six weeks, a simple roller was substituted for the bandage, and the patient was permitted to stand in his room on crutches, but was cautioned against any attempt to support himself on the injured leg. In spite of this precaution, severe pains

having recurred in the fracture, Dr. D'Arcy examined the limb, and was much surprised at finding it shortened by nearly one inch. Muscular contraction, the original cause of the accident, had again caused displacement of the fragments which the day before seemed to have solidly knitted. The resistance met with in the efforts to restore coaptation was so considerable, that fearing the ill-effects of sudden and violent tractive efforts, Dr. D'Arcy applied an apparatus calculated to secure continued extension; he constructed this appliance on the indications of a young American surgeon. It consisted in a jointed box, partially filled with saw-dust; counter-extension was obtained by straps affixed below the knee, and extension was effected by the action of a screw which drew down an iron rod secured to a wooden plate attached to the sole of the foot. Without much effort the fragments were slowly restored to their proper situation, and complete consolidation followed in the course of twenty-five days. A shortening of two or three lines was inevitable, but produced no perceptible lameness.

This case, which throws some light on the mode of production of fractures, also shows that in very oblique fractures muscular contraction may induce secondary displacements, which are to be avoided by keeping the limb in the apparatus for a much longer period than is generally deemed necessary.

PART III.—MIDWIFERY.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

(A) CONCERNING PREGNANCY AND PARTURITION.

ART. 121.—*Cases of Rupture of the Uterus.*

By Dr. ERNST KORMANN.

(*Inaug. Dissertation*, Leipzig, 1864 ; *Schmidt's Jahrbücher*, 1866.)

Dr. Ernst Kormann reports the two following cases, with special reference to their forensic application :—

CASE 1.—A servant, twenty-eight years old, of middle size, rather feeble frame, and moderately well nourished, was admitted into the Leipzig Lying-in Hospital on the 13th of September, 1864. Three years before she had passed through chlorosis, small-pox, and typhus ; and since then her menstruation had been irregular. Two years ago she gave birth to a still living male child with difficulty, but without artificial help. She made a good recovery, and suckled the child for four weeks.

The patient dated the beginning of her then pregnancy from the commencement of January, and was supposed to be in the thirty-eighth week, or middle of the tenth lunar month. Examination showed a transverse position of a moderately-sized foetus, with the head towards the right side and forwards, and the back backwards, and with a large quantity of liquor amnii, rendering the abdomen very pendulous. The foetus remained in the same position until delivery (October 27th). The pains, at first moderately strong, increased by degrees ; and the liquor amnii escaped at half-past two in the afternoon. Version was performed under slight chloroform narcosis. During the operation the patient lay upon her left side, and the uterus was supported from without. The operator guided his hand along the left anterior wall of the uterus to the summit of the cavity, in order to grasp the feet. On account of strong contraction, he only succeeded in securing the right foot. In spite of firm traction, the trunk did not descend ; and Professor Crédé passed his own hand in the same direction into the uterus, drew down the other foot, and completed the extraction, in which only the delivery of the arms was attended with any difficulty, and this was overcome without any considerable loss of time. The head followed by the use of the Prague hand-grip.

The foetus was dead from asphyxia ; it was very large and strongly built, weighed nine pounds five ounces, measured twenty inches and a

half in length, and the diameters of its head and body were somewhat more than normal. An assistant sought in vain for the placenta in the cavity of the uterus; and Professor Crédé, then making an examination, found a rent in the right side of the uterine neck, and through this rent he withdrew the placenta from the abdominal cavity. A towel was bound round the abdomen, and an endeavour was made, by compresses beneath it, to fix down the uterus from above. (At what time the delivery was effected, and how long the various manipulations were in progress, is not stated by the author.)

An hour after delivery the face was pale, forehead and ears cool, pulse regular, rather full, 110, temperature scarcely raised, respirations 28. At evening the respiration was more oppressed, face warmer, pulse 112. On the 28th, very early, after the patient had slept until 1 a.m., there was marked collapse; the face reddened, feeling of anxiety and restlessness, pains in the abdomen, and vomiting. At 7 a.m. the temperature was 30.3° R., pulse full, 108, respiration 32. At 10.45 a.m. the vomiting had recurred frequently; at 1 p.m. it had subsided, but returned once at 6 p.m. The patient had slept a little, but with frightful dreams. After dinner two ounces, and in the evening four ounces, of urine were drawn off by the catheter, rich in lithates, but containing no albumen. In the evening the ears and extremities were warmer. At 9 p.m. pulse 136, temperature 30.3° , respiration 32. On the morning of the 29th the patient felt tolerably well, but had once during the night vomited some greenish watery fluid. Pulse 116, temperature 30.4° , respiration as before; red patches on the cheeks. At 11 a.m. the vomiting returned. After dinner, indications of acute reaction, dry tongue, repeated vomiting, during which gushes of blood passed from the vagina. In the intervals there was a discharge of thin brownish-red fluid. Temperature 31° , pulse 120, respiration 28, abdomen more distended, less painful. 6 p.m., temperature 31.1° , pulse 116, respiration 24. 9 p.m., 31.3° , and 132. In the night one attack of vomiting, and the matter vomited had a fæculent smell. On the 30th, the general symptoms remaining the same, an apathetic condition set in about mid-day. On the 31st, a digital examination showed that the vaginal portion of the uterus had undergone natural involution. It was short, with open orifice, and no portion of intestine could be felt in or above the rent. The finger was found coated by fætid, dark brown, greasy, mucous matter. The abdominal bandage was loosened; and a clyster of soap and water administered, and repeated towards noon. It was followed neither by vomiting nor stool. The facial expression was somewhat easier, the circumscribed redness of the cheeks less. From this time the symptoms of septic metro-peritonitis, with exudation, regularly increased, and the patient died at 7.30 a.m., on the 2nd of November.

Autopsy.—On opening the abdominal cavity, the stomach was found much distended, covering the distended cæcum on the right, and the descending colon on the left, with the sigmoid flexure lower down. Between the lower end of the descending colon, lying in the ileo-cæcal region and the abdominal wall, and commencing two inches and a half above the symphysis pubis was an inclosed collection, containing fætid gas in its upper portion, and below some ounces of dirty-red, stinking fluid. The corresponding peritoneal surfaces were covered by a firmly

adherent substance, a line and a quarter in thickness, partly greenish, partly greyish yellow, and interspersed with a few old blood clots. The descending and transverse colon, and the middle portion of the great omentum, were firmly adherent to the boundaries of this collection in the whole of the right halves of their surfaces. When these adhesions were separated, it was seen that between the convolutions of the small intestine, and in the posterior part of the peritoneal cavity, there was a quantity of dark red, bloody fluid, containing very fine grey particles.

The uterus projected about an inch and a half above the symphysis pubis, and lay close to the anterior wall of the pelvis. It measured 17 centimetres in length, from 12 to 13 in width, and 5 in thickness. Texture very lax. The Douglas's space commenced $2\frac{1}{2}$ centimetres above the lower end; and the peritoneum was absent over a nearly circular space, $2\frac{1}{2}$ centimetres in height, and 3 centimetres in width. In the middle of this space, and (with the uterus in its natural position) looking towards the right posterior wall of the pelvis, was a hole which was stretched by the finger from $2\frac{1}{2}$ centimetres to 5 centimetres in diameter, and which extended downwards through the cervix and os uteri into the vaginal wall. At the bottom of the rent the plicæ palmatæ were clearly visible, much reddened, and covered in places with greenish-grey exudation. To the right and left flaps of uterine tissue projected outwards from the edges of the wound. At the lower part of the rent the serous investment was torn a centimetre and a half below the tear in the cervical tissue, and down to the point where the vagina is inserted into the cervix; so that, when this part also was broken through, the fissure extended into the vagina. The fissure passed from the point of insertion of the posterior vaginal wall into the cervix uteri, a little to the left from the middle line, in a zigzag direction obliquely upwards and outwards to the right side for a length of eight centimetres, measured in a straight line; then bent at an acute angle to the left, and terminated six centimetres from the median line.

No trace of the os uteri internum could be discovered. On opening the anterior aspect of the vagina, by an incision four inches long, it was seen that the rent, although it passed through the whole substance of the uterus, began fully half a centimetre above the inner margin of the external os. In the place of the vaginal portion there was a swollen and fissured ring of very soft and lacerable tissue, from one-half to two-thirds of a centimetre in thickness, œdematous towards the uterine cavity, and gaping to the size of a shilling. The anterior lip was from 1 to $1\frac{1}{4}$ centimetres, the posterior from 1 to $2\frac{1}{2}$ centimetres in length. An incision through the upper part of the fundus uteri displayed very pale, but otherwise normal tissue, from 2 to $2\frac{1}{2}$ centimetres in thickness. From the cavity escaped some turbid red fluid. The external surface of the uterus and its appendages appeared covered with exudation; the vagina very wide, in most places bluish red, soft, smooth, shining; the lower portion and the genital fissure superficially ulcerated. Other organs presented nothing important.

In this case the author believes that the size of the child and the thinness of the right side of the uterus posteriorly must be regarded as the causes of the rupture. It is probable that in the perfectly well-performed version the body of the fœtus would be pressed towards the

right side, and that the uterine tissue would be so much extended, either by the foetal head or arm, as to give way. In the second position of the feet the left arm would pass through the rent into the abdominal cavity. The right arm having been first drawn down, the left was treated in the same way, and, in its passage back through the rent, in a direction downwards and forwards, would enlarge and complete the opening. The disproportion between the thickness of the uterine wall at the fundus and at the seat of laceration must be regarded as a predisposing cause of the accident.

CASE 2.—A married woman, in her twenty-ninth year, and third pregnancy, admitted into hospital on the 31st of May, 1864. She had menstruated from her sixteenth year without pain. On the 31st October, 1859, she was delivered of a full-grown dead female child by forceps, after a lingering labour; and was under treatment for subsequent peritonitis until the 20th of December. On the 23rd of April, 1863, she was delivered of a male child, also dead, with great difficulty, but made a good recovery. When admitted she was in the thirty-seventh or thirty-eighth week of pregnancy. She was a small, but strong woman; and examination revealed much projection of the promontory of the sacrum, with a conjugate diameter of $2\frac{3}{4}$ inches. The child lay in the first position of head presentation. In order to produce premature labour, a bougie was passed into the uterine cavity on the 2nd of May, at 7.50 a.m., after a preceding vaginal injection. By 11.30 weak pains were recurring every three or four minutes. By 8 p.m. they became energetic, and continued so until four the next morning. The os uteri dilated slowly, and the liquor amnii trickled away. From then until 6 a.m. the pains abated, when they returned with great severity and frequency, and by 6.30 p.m. the os was fully dilated. At that time the previously regular and clearly audible heart-sound became feeble; and as some blood had escaped, Professor Credé performed turning under chloroform, and delivered the arms. The head could not be brought through the pelvis, and cephalotripsy was required. The placenta was thrown off spontaneously in three minutes.

Immediately after delivery the patient showed greatly increased sensitiveness. The slightest touch of the abdomen was painful, and the pulse very rapid. Temperature but little elevated. Collapse set in at 10 p.m., and the patient died at 4.30 the next morning.

The abdomen was found to contain about two pounds of fluid blood, together with coagula. The uterus was 7 inches in length, 5 in width, 3 in thickness. On laying it open, a rupture 5 inches in length was found to extend upwards from the external os, and to terminate in a bifurcation.

Kormann infers, from the agitated state of the patient before delivery, that the head had already injured the uterus prior to the turning. The weakening of the heart's sounds was probably due to the escape of blood into the abdomen.

After minutely describing a third case, and after referring to sixty others, the author states his conclusions as follows:—

The maternal predisposing causes of uterine rupture are anomalous conditions of the pelvis (such as pointed exostoses, or unusual sharpness of the promontory); diseases of the uterine walls (such as thinness, local atrophy, fatty change, cicatrices from former Cæsarian section, or from healed rupture, old or recent inflammation, &c., &c.); new growths (such as fibroids, polypi, especially the interstitial and submucous,

and cancer); atresia of the os or cervix uteri; and placenta prævia. There are also predisposing causes due to the foetus, such as abnormal size of the whole or of a single part (*e. g.*, in hydrocephalus), and unnatural positions. The foetus may be so placed that its head or some other part may exert constant pressure on the same portion of the uterine wall, and may thus weaken it so much that it cannot resist the pressure of parturition.

It may sometimes happen that neither the foetus nor the placenta may pass into the abdomen, and that the severe symptoms may be referred to some other cause than the true one, until a *post-mortem* examination reveals the latter.

The exciting causes of rupture may be summed up as all traumatic influences acting upon the pregnant uterus, violent pains and efforts, and undue force in operations for delivery.

ART. 122.—*Rupture of the Uterus—Abdominal Section—
Subsequent Pregnancy and Safe Delivery.*

By EDWARD WHINERY, M.D., of Fort Madison, Iowa.

(*American Journal of the Medical Sciences*, October, 1866.)

Dr. Whinery relates the following remarkable case:—

“On the 28th of March, 1865, at 8 o'clock a.m., I visited Mrs. S., of Niota, Illinois, a healthy Irishwoman, about thirty-seven years of age, who I was told was taken in labour about 10 o'clock a.m. of the 27th. The first indication she had of approaching labour was the escape of the waters, soon after which regular pains supervened, and an ignorant midwife was summoned to attend her. Labour progressed regularly until about seven o'clock in the evening, when it was expected that the child would be born in a few minutes. She was seized at that time with severe burning, lancinating pains, or stitches as she called them, throughout the abdomen, and the expulsive pains immediately ceased. I found her sitting in a chair, leaning forward at an inclination of about forty degrees, and very unwilling to change this attitude. Her pulse was 110, irregular, or fluttering; the countenance very anxious and pale; the skin cool and clammy. It was with difficulty I could induce her to assume a position convenient to make an examination per vaginam. I, however, caused her to be held at an inclination of about forty-five degrees, and passing the digital finger of the right hand into the vagina, and the left over the abdomen, I found the head of the foetus resting well down on the perineum, but by pressing firmly against the head with my finger it ascended above the superior strait, and the whole body could be distinctly felt through the walls of the abdomen, she being of spare habit. The motion thus given to the foetus very much increased the lancinating pains, and she cried out, “These stitches will kill me.” My diagnosis was rupture of the uterus, and I informed her and her friends that her condition was very precarious. The poor midwife said she did the best she knew. She tried to give her ‘Mutterkorn Thee’ (ergot), but the stomach would not take it. The

night was very dark, and the husband and his friends were afraid to attempt to cross the Mississippi in a row-boat, as it was very high, with much drift-wood floating. She therefore spent the night in applying new corn-whiskey to the abdomen.

"I allowed the patient to resume the attitude first mentioned, returned home for my instruments and an assistant. Dr. J. C. Blackburn accompanied me. At 10 o'clock a.m., when we arrived, no change had taken place in the patient. My friend Dr. B. thought, from the visible physical appearances and my representation of the case, that my diagnosis was correct, and we soon agreed upon the propriety of making the abdominal section. Dr. B. administered the chloroform while I was preparing other matters. We placed the patient on her back on a table, and I made the incision on the right of the umbilicus, about six inches in length, through which I removed a large male child (dead of course) and the placenta, both being entirely above the uterus, which was well contracted down into the pelvis. There was very little appearance of hæmorrhage. The rupture was in the fundus from the anterior to the posterior wall. The edges of the wound were now brought together by sutures of silk, taking care to include all the structures except the peritoneum; then finishing the dressing with adhesive straps, a compress, and a wide bandage. The operation and dressing were performed in less than five minutes, and the patient placed in bed still under the influence of the chloroform. When she recovered from its effects she expressed herself as feeling quite comfortable, and grateful for her delivery from her intense suffering for so many hours.

"We expected peritoneal inflammation to supervene, but in this we were happily disappointed. I visited her on the 29th, and found her quite comfortable; the pulse had gone down to eighty, and every symptom was favourable; the lochia was moderate in quantity; she had been nearly free from pain, and slept well during the night, though she had not taken any of the morphia and quina powders left for her in case irritation and debility should supervene.

"March 30 and 31. Continued without an unfavourable symptom.

"On the 3rd of April she sat up three or four hours in bed. The wound had healed by the first intention.

"On the 5th I took out the sutures, but continued the adhesive straps, the compress, and the bandage; she was then dressed and sitting up.

"On the 8th the lochia ceased, and she went about her ordinary housework.

"On the first of June she menstruated, and again on the first of July. Then she became pregnant, and on the first day of this April she gave birth to a healthy female child. I was in attendance, and found the 'waters' had passed off two days before, but there had been no pain until within three hours of the time of my arrival. The os uteri was well dilated, and the head of the foetus was entering the superior strait. Fearing that the former rupture might have impaired the integrity of the uterus, and that we might again have the accident repeated, I applied the forceps and assisted the expulsive efforts, so that in an hour and a quarter after entering her room I had the satisfaction of finishing her delivery. She and her friends were very much relieved, for she

had heard that it was the opinion of some medical men that she could not go through parturition safely after such an accident. There was nothing unusual attending upon gestation. She says this is her tenth pregnancy, and the easiest delivery she ever had; she generally had had difficult deliveries. Two of her children had been still-born in consequence of protracted and difficult labour."

ART. 123.—*Case of Spontaneous Rupture of the Uterus during Labour.*

By Dr. ALEXANDER R. SIMPSON.

(*Glasgow Medical Journal*, June, 1866.)

Dr. Simpson reports a case of spontaneous rupture of the uterus during labour at the full period. The patient was thirty-nine years of age, had been delivered of six healthy children, and in the interval between her sixth and seventh pregnancy she had suffered from extensive ulceration of the os uteri, running into the cervical cavity. The rupture had taken place before Dr. Simpson reached the bedside, and up to this time no suspicion appeared to have been entertained by the nurse in attendance that labour was proceeding otherwise than favourably. The patient had not experienced any labour pains either unusually violent or protracted, but just prior to the arrival of Dr. Simpson they had assumed a character which led the nurse to believe that the labour would quickly end. The os uteri was found slightly undilated, soft and flabby, and a bloody discharge had set in when the labour pains had begun to occur steadily. On the os being further dilated and three fingers passed into the uterus, a large rent was discovered, through which the foetus had escaped into the cavity of the uterus. Signs of rapid exhaustion becoming evident in the patient, gastrotomy was performed and the foetus extracted. The uterus was found collapsed but relaxed, the rent gaping, and through it the placenta was removed. The patient died sixty hours afterwards. An autopsy showed that the intestines were enormously distended with gas, their peritoneal covering being highly injected in some parts of their course, and in the cavity of the abdomen there were a few ounces of the same fluid, of which a small quantity had been withdrawn by the catheter. The uterus was lying in the pelvis, apparently in the very same condition in which it had been felt at the time of the operation; the ragged, irregular, bruised-looking sides of the rent lying gaping apart, so as to expose to view at one point the inner surface of the organ. On removing it from the body, the rent was found to run up precisely along the left side of the uterus, immediately in front of the broad ligament, from the level of the os internum till within half an inch of the Fallopian tube, where it turned somewhat abruptly forward and upward along the anterior wall, and terminated almost at the mesial line, about an inch below the fundus. The laceration in the posterior lip of the os was about an inch and a half in length, involving little more than the mucous layer, and could

be distinguished from some old cicatrices by its ecchymosed margin. The whole substance of the uterus in nearly all its extent was uncommonly pale, soft, and friable—the knife going through it, as was remarked by the gentleman who made the dissection, as if it were a piece of soft pork or lard. On submitting some sections of the organ to the microscope, it was found that the tissues had undergone fatty degeneration to a very remarkable degree. Not only in the inner layers, but throughout all its thickness, the fibres were seen full of granular fatty particles, and the connective tissue between the muscular bundles was unusually opaque.

ART. 124.—*Cases of Laceration of the Uterus, with remarks.*

By THOMAS RADFORD, M.D.

(*Proceedings of the Obstetrical Society: British Medical Journal*, August 11, 1866.)

The author, after briefly alluding to the views of Hunter, Denman, and Douglass, on this most dangerous complication to labour, related minutely the histories of nineteen cases which had fallen under his notice. Of this number, in eleven the ages registered were from twenty-one to forty years, and it was found that the accident occurred more frequently between the ages of thirty-nine and forty. The number of labours which each woman had undergone, varied from the first to the eleventh; and it was shown that laceration of the uterus happened most frequently in women pregnant for the eighth time, and that in those *enceinte* for the first time, the accident took place quite as often as it did in any of the other cases which were registered. The duration of the labour, from its commencement to the occurrence of laceration (though in some cases not exceeding three or four hours) was generally from ten to thirty hours. Of the various causes or conditions mentioned as producing laceration, slight contraction at the brim of the pelvis appeared to have been the most frequent. The author considered that when the form of the pelvis was only slightly contracted, the os and cervix uteri partially descended during labour into or a little through the aperture of the pelvis, so that, as the head of the infant was forced down, the uterine tissues became fixed between this body and the pelvic bones. The fixity of this structure actually formed a *point d'appui* from which the uterine fibres during contraction forcibly pulled; and the great probability was that sooner or later the tissue either directly tore, or, being first contused and softened, yielded. As regarded the situation of the laceration, the cervix uteri was the part most frequently affected, and sometimes with it the body of the organ was also implicated. In eleven cases the laceration was longitudinal, in three transverse, in three oblique, and in one circular. Of the nineteen cases, three recoveries took place, or nearly sixteen or seventeen per cent. Dr. Radford, in his concluding remarks, observed that when we contemplated the frequent fatality of laceration of the womb, we were led to inquire whether there were no symptoms which showed themselves as

universal precursors of this dreadful catastrophe; and if there were, were we possessed of the means of prevention. In all the cases he now brought before the Society, there could not be found any with premonitory symptoms which of themselves would warrant any operative measures being taken, in order to avert the impending danger. Nevertheless, he thought we should carefully consider all the contingent circumstances of protracted labours, and especially of those which were prolonged by mechanical impediments; and whether they were produced by relative disproportion of the capacity of the pelvis to the size of the foetal head; if so, we should adopt measures of timely delivery.

ART. 125.—*Retroversion of the Pregnant Uterus.*

By Dr. LORIMER (Haddington).

(*Edinburgh Medical Journal*, July, 1866.)

Dr. Lorimer relates a case of retroversion of the pregnant uterus ending fatally. The patient was about three months gone in pregnancy, and the first indication of disturbance was difficulty of micturition coming on in the night, after she had gone to bed apparently in good health. There was much abdominal tenderness and distension, also considerable exhaustion and emaciation when first seen. From the position of the womb much difficulty was experienced in passing a catheter into and fully relieving the bladder. Abortion was induced by the use of the uterine sound, followed by the administration of ergot, but the general and local symptoms were not alleviated. Every attempt to replace the uterus both before and after abortion failed, and eventually the patient died. On an examination of the body the uterus was found completely retroverted, and a fibrous tumour, the size of a small orange, was lodged in the posterior wall.

ART. 126.—*Case of Inversion of the Uterus.*

By Dr. JAMES SIDEY.

(*Edinburgh Medical Journal*, September, 1866.)

On the 20th of May, Dr. Sidey was sent for to see Mrs. G., who was five months advanced in pregnancy, and found her complaining of pelvic pain. On examination, the parts were all very tender, and the os uteri could scarcely be reached on account of extreme tightness. The pain continued till the 27th, when suddenly, without any faintness, there was a feeling of extreme distension and fulness, the abdominal tumour reaching above the umbilicus in the evening. Uterine pains came on, when a large clot was expelled, and within the neck, the placenta was felt firmly adhering; no more hæmorrhage, however, occurred. On the 28th, the uterine pains again recurred, and the foetus and placenta were

expelled very much blanched, except the portion which had evidently adhered to the neck and lower part of the uterus. On examination, a large tumour was felt, which proved to be an inverted uterus caused by a fibrous tumour forcing its way through the os. Two fingers of the right hand were passed upwards on the rough surface of what appeared to be the fundus, at the same time pulling the tumour up with the other hand until an os appeared to have been formed, and the uterus assumed a natural state. Since then she has done well.

ART. 127.—*Case of Inversion of Uterus after Delivery.*

By Dr. DENHAM.

(*Dublin Quarterly Journal of Medical Science*, August, 1866.)

Out of a hundred thousand deliveries that have taken place in the Dublin Lying-in Hospital since its foundation, only a single instance of acute inversion of the uterus is recorded. This solitary case occurred during the mastership of Dr. Shekleton, and is given in the report of the hospital published by Drs. Johnston and Sinclair.

The subject was nineteen years of age, thin and delicate. She was delivered of her first child after an easy labour of six hours. Some slight pressure having been used by the attendant, the uterus was found suddenly to recede from the grasp, and was immediately expelled from the vagina an inverted mass, with the placenta still attached to it. The patient became pallid, almost pulseless, and exceedingly anxious; complained of considerable pain and a sense of sinking.

The placenta was easily separated without hemorrhage, and the uterus returned with but little difficulty in about seven minutes. No bad symptoms followed, and she was discharged in a short time, quite well.

After thus illustrating the rarity of the accident from the statistics of the Dublin Lying-in Hospital, Dr. Denham relates the following case which came under his own observation:—

“Jane Savage, aged twenty-three, admitted into chronic ward 12th January, states that she was delivered of her first child five weeks before admission. The membranes ruptured, and the waters kept dribbling away for two days before labour set in; on the third day there was a red discharge, and labour pains came on that evening. She was weak and faint during the night, and was delivered at seven o'clock the following morning, with only three or four expulsive pains. The placenta was forcibly extracted by the midwife in about ten minutes after the birth of the child, both by traction on the cord and pressure on the fundus of the uterus. Some difficulty or delay seems to have been experienced in its removal, for the patient described the nurse as having twisted something like a cord round the wrist of one of her hands, with which she made considerable traction, while at the same time she strongly pressed on the belly with the other. During this time she suffered a great deal of pain, but suddenly got relief by the expulsion of a large tumour from the passage, which led her to exclaim,

“Oh, Mother of God! am I going to have another!” The placenta, which was partially detached, was now entirely separated, and the uterus thrust into the vagina. The poor patient remained weak and exhausted all the day, passing from one attack of syncope to another, until four o’clock, when she was seen by the dispensary doctor, who, unfortunately, only felt the pulse and looked at the patient, but made no examination. She remained in bed for eleven days, and then got up a little every day until her admission; the doctor saw her twice, but never made a vaginal examination; in fact, he never diagnosed the nature of the case. During all this time she had a continual shedding, especially at night.

She came into hospital in a most pitiable condition—pale, weak, and exhausted, from the combined loss of blood, appetite, and rest. In making an examination it was thought that a simple case of polypus had to be dealt with; but on passing the finger round the neck of the tumour the os uteri could not be discovered, while at the same time the tumour wanted the smooth polished surface generally met with in such cases; conclusion, therefore, was arrived at that it must be a case of recent inversion of the uterus. A generous diet, with a liberal supply of wine, was ordered. Citrate of iron and quinine, with an anodyne at bed-time was prescribed, with perfect rest in the horizontal position. The poor woman greatly improved under this treatment, so an attempt at reduction was made on the fourth day after her admission. Having been brought fully under the influence of chloroform, she was placed on her back, with the thighs flexed on the pelvis, and the legs on the thighs. The hand was then slowly introduced into the vagina, and the fundus and body of the inverted uterus firmly grasped with the fingers and thumb. Steady gentle pressure in this way was brought to bear on the entire tumour for several minutes before any attempt was made at reduction. The tumour gradually diminished, partly from the pressure and partly from the loss of blood, which was very considerable. Pressing steadily upwards, the uterus was now felt gradually to yield, and in a short time the fundus alone remained unreduced; no amount of force, however, compatible with the safety of the organ could enable us to complete the operation, and as the patient was faint from the loss of blood, no further attempt at reduction was made at that time. The vagina was now syringed out with cold water, the patient was replaced in bed, and a full anodyne administered. On the following day she had a rapid pulse and complained of pain and tenderness over the uterus. Opium, with small doses of mercury, was freely administered, and linseed poultices were kept over the abdomen constantly.

The symptoms gradually subsided; and on making a vaginal examination on the third morning after the operation, it was discovered that the fundus had spontaneously returned either by its own elasticity or the contraction of its muscular fibres. For many days there was a very profuse purulent discharge from the uterus, but the patient steadily improved in appearance and health, and was able to get up for a few hours every day at the end of a fortnight. On examining with the speculum about a week after the reduction the os uteri was found ragged and inflamed, but the sound passed up readily into the cavity of the uterus without causing much pain or uneasiness. She was dis-

charged from hospital in perfect health, having menstruated regularly a few days before leaving.

ART. 128.—*On Morbid Conditions and Injuries of the Spleen in the Pregnant and Parturient States.*

By Sir JAMES SIMPSON, Bart.

(*Edinburgh Medical Journal*, September, 1866.)

In a paper read before the Obstetrical Society of Edinburgh, Sir J. Y. Simpson referred to three cases of fatal rupture of the spleen, which had occurred respectively in the pregnant, parturient, and puerperal states. He pointed out the circumstance that, during pregnancy, there is often, if not generally, an increase of the white particles of the blood, or, in other words, a kind of normal or physiological leucocythemia. As in states of morbid leucocythemia, the spleen was often enlarged; so was it also occasionally in pregnancy. Perhaps it would be found in practice much more common than the silence of authors on the subject might lead medical men to suppose. It sometimes recurred in successive pregnancies. In one patient of his, the spleen became enlarged to a very marked degree in a series of successive pregnancies, and this splenic enlargement disappeared always after delivery. Her youngest child is now about ten years old, and during that time there has been no recurrence of the splenic hypertrophy in the mother. A certain amount of softening very frequently accompanies the hypertrophy of the spleen, and predisposes to the laceration of the organ under strong exertion and muscular effort, blows, &c. The first case of rupture of the spleen in a child-bearing mother which he saw was a patient of Dr. Husband's. She began to show symptoms of fatal sinking shortly after premature labour set in, about the sixth or seventh month. On opening the body after death, the enlarged spleen was found lacerated, with effusion of blood into the peritoneal cavity. Shortly afterwards, a patient of Dr. Wilson's who had been delivered a week or two before, after making some unusual muscular exertion, complained of abdominal pain and sinking, and died. Rupture of the spleen and effusion of blood were found on dissection. The late Dr. Cunningham, of Currie, delivered a patient in Edinburgh, using the forceps. He left very shortly afterwards to catch the railway train. The patient sank and died within an hour or two. An inspection of the body was ordered by the law authorities, when rupture of the spleen, and consequent effusion of blood, were found to be the immediate cause of death.

ART. 129.—*Rupture of a Varix in the Vagina.*

By F. W. HELFER.

(Schmidt's Jahrbücher, 1866.)

The unusual character of this case, which was reported by the author to the Obstetrical Society of Leipzig, gives it an especial interest.

The patient was a multipara, thirty-five years old, who during her last pregnancy suffered from an unusually extensive varix of the left lower extremity. Her delivery was natural; and her progress afterwards until the twenty-first day, when a gush of blood took place from the vagina, and was traced to a fissure in the upper part of its posterior wall. The bleeding was checked by a tampon and astringent, but returned repeatedly, and caused the death of the patient on the twenty-sixth day. At the autopsy, two rents were found on the posterior wall of the vagina, leading into a burst varix. One of these rents, half an inch long, and two inches and a half from the outlet, was directed towards the latter; the other, from an inch to an inch and a half in length, and situated rather higher, extended under the pelvic fascia, so that the index-finger could be passed into a cavity, filled with extravasated blood.

ART. 130.—*On the Hygiene of Lying-in Hospitals.**(Journal of Practical Medicine and Surgery, June, 1866.)*

A debate on this subject occurred in the course of a discussion in the Chirurgical Society of Paris, in which Messrs. Velpeau, Le Fort, Tarnier, Guyon, Blot, Boinet, Trelat, and Chassaignac took part. The Society has summed up the results of the discussion in the following manner:—

1. It is now fully demonstrated by statistical returns that puerperal affections are far more frequent, and the mortality much more considerable, in lying-in hospitals than elsewhere.

The invariable reproduction of the same facts in every institution and in all countries, shows that they are referable to one influence, that of the hospital.

It is therefore highly desirable in all parts of Europe to extend as much as possible the gratuitous practice of midwifery at the homes of the poor, and gradually to suppress lying-in institutions.

2. The increased mortality, which sometimes reaches a formidable degree of intensity, and is habitually ascribed to the prevalence of epidemic disease, is almost exclusively referable to two causes, viz., the deleterious atmosphere of hospital wards, and perhaps the contagious character of puerperal affections.

The sad effects of hospital influence explain how it happens that in the best constructed, most healthily situated, and most thoroughly ventilated establishments, the mortality is often considerable, and sometimes excessive.

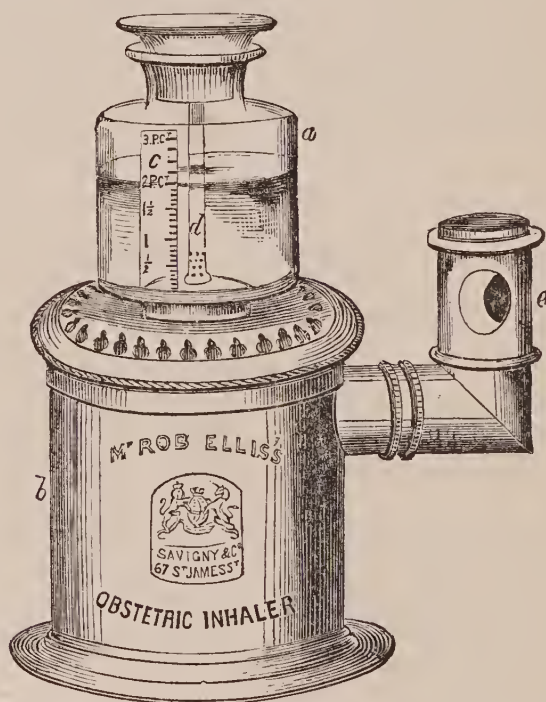
3. In addition to the general rule of hygiene applicable to all nosocomial institutions, and propounded by the Imperial Society of Surgery, the prophylaxis of puerperal maladies should be directed especially against infection and contagion, by disinfection of beds, linen, furniture, separation of the sick, avoidance of overcrowding, &c.

ART. 131.—*Mixed-Anæsthetic Inhaler for Obstetric Practice.*

By Mr. ROBERT ELLIS, Obstetric Surgeon to the Chelsea, Brompton, and Belgrave Dispensary.

(*The Lancet*, June 30, 1866.)

Mr. Ellis describes a new and simple mixed-anæsthetic inhaler, with especial reference to the lying-in room.



The instrument is represented in the cut. It consists (the face-piece, &c., being all as usual) of two parts—an upper, which holds the chloroform or other anæsthetics, and a lower for the alcohol and general evaporation of the fluids. The upper part is an entirely new little apparatus which Mr. Ellis has designed and perfected for dropping chloroform in an equal ratio per minute: this he calls the “chloroform-dropper.” It consists of a glass bottle of a peculiar construction (*a*), perforated at both ends. Up its centre runs a double metallic tube (*d*), the inner one of which contains a few strands of wool, and the outer serves as a cap to prevent spilling and irregular action of the apparatus. This is held in its place by a cork fitted into the lower end of the bottle.

The upper end has a flat stopper, removable to allow of pouring in fresh supplies of fluids. Now, when a definite quantity of chloroform is poured into this bottle, the wick immediately causes it to rise up the tube, and descending within it, the fluid appears below in regular drops. The rate at which the fluid comes over is easily learnt by calculation, and there is an engraved scale (*c*) on the side of the bottle which accurately informs the operator how much fluid per minute is escaping from the bottle, and the consequent per-centage of chloroform vapour which his patient is inhaling. Nothing more is necessary in order to give chloroform at a rate of from one to three per cent. than simply to pour the fluid, at intervals of five or ten minutes, into this little reservoir, up to the indicated mark for the per-centage requisite. The beautiful regularity with which this most simple instrument performs its required office must be seen, Mr. Ellis states, to be fully appreciated. At its highest power it can only give off three per cent. of chloroform. Absolute security, he avers, is thus obtained, while—a much lower per-centage being, in his opinion, quite sufficient—this can be perfectly secured by keeping the fluid at a certain point by occasional replenishment. Its tendencies, also, are all in the direction of safety, for its rate diminishes, with great equability, from three per cent., until, if left unsupplied, it finishes with about a half per cent. of the chloroform. Thus the patient, once anæsthetised, may be kept in that state with the greatest safety, since at every few minutes she derives a smaller and a smaller quantity of fluid from the apparatus. The lower part consists of a simple metal cylinder (*b*), the interior of which is occupied by my arrangement of leaf-like processes for the evaporation of alcohol. Upon this fimbriated structure the chloroform drops, and passes with the alcohol and ether vapour into the respirations of the breather. To this part of the apparatus is attached the elastic tube and face-piece. These have been well made by Messrs. Savigny, the makers of my other apparatus.

In actual practice the instrument is thus used. A certain portion of pure alcohol is poured upon the evaporating surface in the cylinder; this lasts for about ten minutes. The chloroform-dropper is supplied up to the required mark on the scale with the fluid, and immediately delivers its contents drop by drop over the evaporator, on to which a few minims of ether are occasionally poured; or ether and chloroform in equal parts may be poured into the dropper, and coming over in single drops are dissipated into vapour with wonderful regularity. But the alcohol cannot be thus mixed; it is always to be poured on to its own proper surface.

The instrument is thus entirely self-acting, and to a great degree self-supplying. It is incapable of derangement by agitation or otherwise; it affords a perfect security to the patient, who cannot draw from it more than a known (and visible) per-centage of chloroform; and it is almost as simple as the commonest kind of inhaler.

ART. 132.—*On Prolapse of the Funis as an indication of Deformity of the Pelvis.*

(*Journal of Practical Medicine and Surgery*, May, 1866.)

The following three cases, which occurred in the Lying-in Hospital, Paris, are cited in illustration of a presumed connection between deformity of the pelvis and prolapse of the funis.

The first patient was delivered seven years before with the forceps, and gave birth to a living child. On the present occasion, at the conclusion of a second pregnancy, she was conveyed to hospital on account of presentation of the cord. One of the midwives of the hospital succeeded in carrying up the cord beyond the head, and the heart of the fœtus continued after the operation to throb feebly, but distinctly. The antero-posterior diameter of the pelvis was three inches and a quarter, being about fifteen lines below the average. Despite these unpromising circumstances, the forceps was applied, and both mother and child are doing well.

In another case of prolapsus, the diameter was three inches and three quarters, thus offering dimensions somewhat more normal, but still indicative of marked constriction. The fœtus having ceased to live, the perforator was resorted to.

In the third patient, procidence of the cord coincided with presentation of the hand, the conjugate diameter barely exceeding three inches. The child was still-born, and was extricated with the forceps.

ART. 133.—*On the Treatment of Tedious Labour in the Second Stage.*

By DR. J. THORBURN (Manchester).

(*The Medical Times and Gazette*, September 1, 1866.)

In a paper read before the British Medical Association, the author first referred to the strong denunciations of "meddlesome midwifery" contained in all modern British text-books. These, the natural reaction from an opposite state of things, had in many points been carried too far, and had led to a discrepancy between the practice and preaching of eminent accoucheurs. For the sake of testing this, he discussed the appropriate treatment of a case where the presentation is natural, the os well dilated, the head begun its descent, there is no manifest obstacle and the parts seem apt for delivery, the uterine pains are of average strength and frequency, and there is no constitutional disturbance, but matters have been *in statu quo* for a couple of hours or thereabouts, and there is a strong probability of lingering labour, though it may ultimately terminate naturally. Such cases occurring frequently, there are three courses open—to trust entirely to nature, to give ergot, or to use the

forceps. The first course is authoritatively laid down by all recent British writers as the most natural, and therefore the safest. To set against this view, there is the certainty of much longer suffering, the possibility of exhaustion, which may be suddenly developed, increased risk of metritis from over-exertion, of sloughing of the soft parts, of hæmorrhage from inertia, of rupture of the uterus, and the possibility of having to fall back on one of the other plans under less favourable conditions. None of these events may be very likely, but they are all possible; and any procedure which will diminish one or all of these dangers without substituting equal risks, should be adopted. Ergot does not, in the author's opinion, fulfil this condition; for, although it may succeed in speedily terminating the labour, it may also fail, and involves a greater risk to the child from spasmodic or unintermitting pressure, or from its poisonous action (?), and it greatly increases the danger of rupture of the uterus and retained placenta. By forceps, speedy delivery is certain, undue uterine fatigue is obviated, and all chance of rupture from straining is abolished. The author endeavoured to show that, in proper hands, there is no corresponding disadvantage; that sloughing of the soft parts and torn perinæum, so far from being more, are less probable; that there is no risk in thus carefully and slowly emptying the uterus; that the pain of labour diminished; and that the risks to the child are lessened in such a case. He protested against the invariable rule of arguing the question on the ground that the operator may be unskillful; such an argument never being used in discussing the general advisability of any other surgical proceeding. He concluded that, contrary to the opinion of Murphy and others, there are cases in which it is advisable to interfere merely for the purpose of abbreviating labour, and that the practitioner is not liable, in such a case, for any consequences that ensue, except such as are clearly traceable to his own neglect or maladroitness; and agrees with the American accoucheur Hodge in emphatically condemning the "practice which permits the agonies of labour to be unnecessarily prolonged, or the safety of the mother or child to be jeopardised, from the timidity or ignorance of the accoucheur respecting an agent whose employment necessarily involves no danger."

ART. 134.—*A Case of Embolia of the Pulmonary Artery in a Puerperal Woman.*

By Dr. F. RITTER.

(*Monatsschr. f. Geburtsk.*, February, 1866; *British and Foreign Medico-Chirurgical Review*, July, 1866.)

In this case the patient was pluriparous, æt. twenty-six. The first day of childbed went off quite regularly. Then the lochia became offensive, and slight pain was felt on pressure. Temperature, 38·0° C. These symptoms disappeared eight days afterwards. She suddenly fell back in deep syncope. Consciousness did not altogether leave her.

When seen the face was pale: she seemed dying. Respiration not much accelerated; pulse very small and frequent. She complained of oppression in the chest. Next day these symptoms persisted; the temperature was 36.8° C.; respirations were catching in character, 36; cyanotic lips and tongue. Subsequently, pulse and respirations increased in frequency; temperature fell. She died the third day. *Autopsy*—In the right horn of the uterus was an abscess; inside uterus no disease. In ovarian veins, small yellow fibrin-clots, apparently formed before death. In the right chief branch of the pulmonary artery was a pale-red delicate thrombus, plugging the vessel.

ART. 135.—*Case of Hypertrophy of the Labium.*

By Dr. MEADOWS.

(*Proceedings of the Obstetrical Society: British Medical Journal*,
September 8, 1866.)

The patient, twenty-seven years of age, about six weeks after her second confinement, three years since, experienced sudden pain and enlargement of the labia. Treatment was adopted, and in 1864 she was admitted into St. Bartholomew's Hospital, where drainage-tubes were passed through the mons veneris and left labium; but she left the hospital unrelieved. When seen by Dr. Meadows, the left labium was the size of the fist, and the whole mons was much thickened, indurated, and brawny-looking. On July 20 the entire labium was removed, together with an elliptical portion of the mons. The operation was successful, and left the parts on that side of their normal size.

ART. 136.—*Menstruation in Pregnancy.*

By GRAILY HEWITT, M.D., Professor of Midwifery, University College.

(*Proceedings of Obstetrical Society: British Medical Journal*,
August 25, 1866.)

The following case is related by Dr. Hewitt as illustrative of the occurrence of menstruation in pregnancy, and as a contribution to the knowledge of this subject:—A. B., aged upwards of thirty, had several pregnancies. The last child was born June 23rd, 1865; suckled one month. The catamenia appeared from September 15th to 25th; in October they were absent; on November 7th she had a discharge of blood, with slight watery discharge, alternating for a week. December 7th, she was "poorly," as usual, for six days. January 8th, 1866, she felt quickening. March 1st, pregnancy was distinctly diagnosed. Delivery of a female child, apparently about a fortnight short of full time, took place on May 17th. The author considered it probable in this case that there was a twin conception, one ovum perishing and giving rise to the

flooding observed in November. It might be that some other cases of apparent menstruation in pregnancy have a similar source ; but in regard to the majority of the cases of menstruation in pregnancy, and excluding cases of irregular hæmorrhage, he believed the source of the blood to be the decidua vera, as in ordinary menstruations, the unusual condition in such cases being the absence of adhesion of the two membranes, the decidua vera and decidua reflexa. The decidual chamber may, in other words, persist to a later period than usual, in which case there is no difficulty in accounting for the exudation of blood from within it, and its appearance externally.

ART. 137.—*On the Age of Nubility.*

By Dr. J. M. DUNCAN.

(*Edinburgh Medical Journal*, September, 1866.)

Dr. Duncan considers the age of about from twenty to twenty-five the nubile age of women. The numerous facts and arguments he has adduced, appear to him to bear out distinctly this conclusion. Below twenty years of age, woman is immature, she runs considerable risk of proving sterile, and if she does bear a child she runs a comparatively high risk of dying in childbed ; besides, her early marriage brings many other disadvantages. The woman above twenty-five years of age is mature ; but to counterbalance this, she encounters some greater risks than the very young wife's, though of a similar nature.

(B) CONCERNING THE DISEASES OF WOMEN AND CHILDREN.

ART. 138.—*Case of Vicarious Menstruation.*

By Dr. MASON, Ayr.

(*Edinburgh Medical Journal*, September, 1866.)

Dr. Mason places on record the following singular case :—

“About the middle of March of the present year, I was requested by my friend Mr. Haldan to see a patient whom he had been attending for two or three weeks, but from illness was unable at the time to continue his visits. The patient is a young lady, fifteen years of age, residing at a boarding-school in this town ; her native place being Liverpool. On calling, I was furnished by the lady of the house with some of the previous history of the case, with which I think it would be better to begin.

“When eight years of age, Miss — first began to menstruate, and continued to do so regularly until eleven ; menstruation then ceased, and did not reappear until she was thirteen, since when, up to the middle of February, 1865, it continued regularly. At that time Mr. Haldan was requested to see her, and found what appeared to be a large

abrasion of the cuticle in the middle of the right cheek, suppurating in the centre, and inclining to bleed towards the circumference. This sore was exceedingly obstinate, refusing to yield to the local and constitutional treatment resorted to. As far as I can gather, dilute nitrate of mercury ointment, caustic, &c, were applied, and cod-liver oil and iron exhibited internally.

"During the summer, Miss — went to Liverpool, her face still unhealed, and, I believe, menstruation very irregular. She was then attended by a medical gentleman; but her face continued so bad, that she did not return to Ayr until the winter. Her medical attendant in Liverpool used locally a solution of sulphate of copper, and covered the part with goldbeaters' skin. Of his constitutional treatment and other *local* applications, I am not prepared to speak with accuracy, as the young lady could give me no clear account of what had been used. From the time her face healed (which I think was in October) until I saw her in the following March, she menstruated every month, the discharge lasting six days each time, and being profuse.

"When I saw her she had a large *patch* on her right cheek close under the lower eyelid, and extending from the outer border of the malar bone to the side of the nose, and about three-fourths of an inch in breadth. On examining it, it appeared as though the cuticle had *melted away*, and numerous little specks of blood were seen on the surface, which was quite wet with a thin serous discharge. An hour before I came, she exclaimed, 'Oh, I feel another place on my face again,' and *immediately* the above appearance was observed. The occurrence of these patches is accompanied by a severe burning pain in the part, lasting for two or three hours. Until very lately, she had not the slightest intimation beforehand that another place was about to break out; the suddenness with which they appeared being almost incredible. Latterly, I observed her lean her head upon her hands, and wear an almost anxious look; and on questioning her, she said she felt rather giddy, and in a quarter of an hour or less another place would break out. It is remarkable that these outbreaks *generally* took place about the same time each day—eleven a.m. Sometimes they occurred in the afternoon, but *by far* the majority at the time specified. As each day almost some new patch appeared, I was very anxious to be present at the time they occurred, and learning the regularity with which they appeared at eleven in the forenoon, timed my visits accordingly. The next day, as I was dressing my patient's face, she exclaimed, 'Oh, I feel a place on my arm.' I at once turned up her sleeve, and there was a large oval patch, fully two inches in length and one in breadth, on her left forearm, presenting the usual appearances. Here I should mention that these patches assume two different aspects at the outset; sometimes the one and sometimes the other obtaining. The one at the outset appears like a dew of blood, the other has a greater tendency to a serous discharge ending in suppuration. Those that bleed most heal the soonest. But before the places heal (which generally takes place in five or six days), both suppuration and hæmorrhage often occur in the same place.

"The hæmorrhage, I should observe, does not consist merely of the dew of blood referred to—that is only at the outset—but it is actual

bleeding as from a cut, the blood sometimes streaming down the face or other part attacked. The worst place she ever had was on the chin; it did not heal for nearly four weeks, and suppurated freely, the bed-clothes in the morning being often soiled by the discharge, but it also at times bled considerably. As soon as one place was healed it broke out in another, or in the same place over again, some of them having occurred in the same place four or five times. It were tedious and useless to describe all the places that were affected, as all were so similar; suffice it to say, that her face was covered, her chest twice attacked, and both arms and legs.

“For some time I was much at a loss to satisfy myself as to the true nature of the case, but finally came to the conclusion that it was vicarious menstruation. During the course of her attack, I sent Miss — into Glasgow to see Dr. McCall Anderson, and he formed the same opinion of the case as myself, and kindly suggested to me, in a letter subsequently, some alterations in the treatment, to which I shall presently allude.

“While still suffering from the complaint, Miss — had a severe attack of whooping-cough, which seemed greatly to aggravate the patches on her face, causing them to bleed freely. This, I have no doubt, was caused by the mechanical exertion during the paroxysms of coughing sending the blood to the face. At this time also she had frequent and copious epistaxis, generally after a fit of coughing, or after the retching thereby induced, and this *somewhat* relieved the parts attacked.

“A few words now as to the treatment. When I saw Miss — she was then using the solution of sulphate of copper to the original spot in the centre of the right cheek, but had not yet applied anything to the new place which had just appeared an hour before my visit. I sent for some oxide of zinc powder, and dusted it well over the part affected, and then covered it with goldbeaters' skin. To the original sore I continued the solution, and so could compare the effects of the two applications. The solution caused a good deal of smarting, which continued for some time after its application; but no inconvenience was experienced after the use of the powder. I tried the solution to some new parts, but it only seemed to aggravate them. The original sore was, however, healed by it, but this part from the first differed from all the subsequent ones, as it penetrated much deeper and suppurated very freely for a long time; it is the only place where any scar is left, and it is trifling. Each morning I removed the goldbeaters' skin that I had applied the previous day, and, after bathing the part with tepid water, carefully removed the scabs that had formed, so as to prevent the occurrence of cicatrices. The places that appeared on the chest and arms I treated somewhat differently. On their appearance I bathed them with cold water, and then applied glycerine, and dusted the oxide of zinc powder over it, so as to form a crust; the arms were then loosely bandaged. This plan succeeded admirably on the arms and chest, but did not answer well on the face. Very few scabs formed on the patches on the arms, and they did not bleed so much as those on the face, and healed much more rapidly. The parts affected on the legs bled freely.

"Internally, she got cod-liver oil and the muriated tincture of iron, with liquor arsenicalis. Aloetic purgations were also exhibited, so as to keep the bowels freely open, especially at the time that *any* appearance of menstruation occurred. A hot mustard hip-bath and leeches to the inside of the thighs were employed at the suggestion of Dr. M'Call Anderson, and I think with much benefit.

"In conclusion, let me very briefly recapitulate some of the most striking points in this case.

"In the *first* place, we notice the very peculiar appearance presented by these spots; the thin serous discharge with numerous specks of blood seen in some of them; and the copious dew of blood, followed by actual hæmorrhage in others.

"*Secondly*. The instantaneousness of their appearance; the skin appearing perfectly whole and healthy one second, and melted away and bleeding the next. It being only lately that any giddiness betokened their appearance.

"*Thirdly*. The almost uniform regularity with which they occurred about eleven every forenoon.

"*Fourthly*. The pertinacity with which patch after patch succeeded one another, and the obstinacy with which they so long refused to yield to the influence of remedies.

"Miss — has now been quite free from any spots for about six weeks, and no traces of them are to be seen, except when she gets heated or excited, and then the parts that have been attacked look very red. The original spot has left a small depression, but little noticed. And now comes a singular fact; and that is, that although healed and apparently well, her menstruation is not yet properly established.

"During the period that I was attending her, she menstruated *one day every week* for four weeks, there being, however, very little appearance. Then a fortnight would intervene without any menstruation, and then it would begin again as before. And now that she seems perfectly well, I learn that the menstruation is still being carried on in the same manner, the discharge, however, each day of its occurrence being more copious. She is still continuing the cod-liver oil, and has resumed the iron and arsenic, which had been omitted for a short time. On calling two days ago, I was told that Miss — had felt dizzy, and that some of the old spots on her face were looking red and angry; I accordingly ordered leeches to the insides of the thighs, and the threatened attack seems to have passed off. But until regular menstruation be established, I shall not be surprised at a recurrence of the attack."

ART. 139.—*On Medicated Pessaries.*

By Dr. KIDD, Assistant-Physician, Coombe Hospital, Dublin.

(*Dublin Quarterly Journal of the Medical Sciences*, November, 1866.)

At a meeting of the Dublin Obstetrical Society, Dr. Kidd exhibited a new form of medicated pessary, made for him by Mr. Pakenham, of

Henry-street, Dublin. Dr. Kidd reminded the members that, on the 13th January last, he exhibited some pessaries made in Edinburgh for Sir James Simpson's use, and some of the same description made by Mr. Pakenham. These were composed of cocoa-nut butter, a substance sufficiently firm at low temperatures to allow of the pessary being introduced into the vagina by the patient herself, but which melted very rapidly at the temperature of her body; but he then said that in practice he had found these pessaries were often objected to by patients, because, as the cocoa-nut butter melted it escaped from the vagina, and soiled the clothes in a very offensive manner, and that, to obviate this difficulty, he had lately got the pessaries made as small as suppositories, so that, instead of containing 120 or 150 grains of cocoa-nut butter, they contained only five or ten. But a pessary of this size would not contain a sufficient dose of many of the medicinal agents that may be applied to the uterus with advantage, and it is difficult to properly introduce such small pessaries into the vagina; so that it becomes necessary for the medical man to apply them himself, by means of the speculum. This latter objection, Dr. Kidd said, applied also to another means of applying medicinal substances directly to the uterus, viz., that suggested by Dr. Tilt, by wrapping them up in cotton, and introducing it into the vagina, as this can only be done rightly by means of the speculum.

The requisites, Dr. Kidd said, for a good form of medicated pessary are:—

1st. That it can be introduced by the patient herself.

2nd. That it will bring the medicinal agent into contact with the mucous membrane of the vagina and uterus, and retain it there sufficiently long to allow of absorption, or of such local action as may be required.

3rd. That it will not be offensive to the patient, soil her clothes, or prevent the medicine having due effect, by allowing it to escape from the vagina.

The pessary now exhibited by Dr. Kidd complied with all these requisites, and had none of the disadvantages of the other forms, nor did it require the use of an instrument for its introduction into the vagina, like that recently suggested by Professor Raciborski. It was composed of cotton, coated with an exceedingly thin layer of cocoa-nut butter, so thin and so small in quantity as to be unobjectionable. In manufacturing this pessary, Mr. Pakenham rolls a small portion of cotton, to which a thread has been attached, on the end of a glass rod, giving it the form and size of the ordinary conical medicated pessary. He now dips it rapidly into melted cocoa-nut butter, so as to give it a uniform thin coat, to preserve its shape, and give it firmness. As soon as the cocoa-nut butter is cool, the glass rod is withdrawn, and the pessary now resembles an empty cartridge case, which may be charged for use by introducing into it the medicinal agent to be employed, and which may be used either in the state of dry powder, or mixed with glycerine or water. The end of the charged cartridge is now plugged with cotton and cocoa-nut butter, and it is ready for use.

It has now a form and consistence to allow of its easy introduction into the vagina by the patient herself. The layer of cocoa-nut butter

soon melts, and mingles with the cotton, serving to bring the drug with which the cartridge is charged into contact with the vaginal mucous membrane, and allow of its action on it. When the pessary is introduced, the thread which has been attached to the cotton is allowed to hang out of the vagina, and serves as an easy means of withdrawing the cotton, when a sufficient time had elapsed for the action of the medicine.

Dr. Kidd stated that he had used these pessaries charged with various substances, such as tannin, alum, iodides and bromides of potassium, iodine dissolved in glycerine, acetate of lead, opium, morphia &c., and had found them most satisfactory; but he had learned that active preparations, such as tannin, which may irritate the mucous membrane, or morphia, should be used in smaller doses than in the old form of pessaries.

ART. 140.—*On the Management of Weak New-born Infants.*

By Professor DEPAUL.

(*Journal of Practical Medicine and Surgery*, October, 1866.)

Professor Depaul remarks that while abundant attention is given in obstetric treatises to the treatment of healthy new-born infants, and those who are seemingly still-born, little space is devoted to the care of the weakly. This want he endeavours in part to supply. He thinks that authors have not laid sufficient stress on certain deceptive appearances, which seem to imply that the infant is out of danger because it takes the breast and seems to suck.

The fact is, however, one of very common occurrence; the infant apparently sucks, but does not increase in weight, and after a time discontinues its fruitless efforts, screams more frequently, and wastes away. In order to discover whether suction is efficiently performed, the child should at the time he appears to be taking the breast with most vigour, be removed from its nurse, and the presence or absence of milk in its mouth be ascertained. The paid nurses at the hospital are required every day to make this experiment. Mr. Depaul also endeavours by all means to rouse from their indolence the wet nurses to whom puny, delicate infants have been intrusted, when the nursling takes the breast but imperfectly. Under these circumstances, it often happens that the infant has not strength to suck, and the finest nurses are provided in vain. The best nurse in such cases, is not the woman who has the largest supply of milk, but one whose milk flows easily and drops without effort into the child's mouth. If a nurse of this kind cannot be procured, milk of good quality should be obtained and given mixed with thin gruel. Mr. Depaul agrees with Professor Scanzoni, that ass's milk is the best for the purpose, but in most cases the practitioner must be satisfied with cow's milk. Every hour or two, day and night, from one to three teaspoonfuls of diluted milk should be administered. Should this kind of food give rise to colic, Scanzoni recommends the addition of a little fennel or dill water; and as soon as the child has gained in

strength, it is proper to procure for it a good wet nurse, and this should not be too long delayed, lest the habit of receiving nutriment into its mouth without any effort may prevent the infant ever taking to the breast again, a circumstance which occurred in the case of a young prince, at present living in exile; the nurse should then be instructed to draw her own milk with an exhausting glass; but this can seldom be obtained from a mercenary nurse, and scarcely ever succeeds but with mothers who rear their own children.

It should further be remarked, that in primiparæ the nipple is often so large or so hard, that if the child is not very strong its efforts at suction are unavailing. The mother is then in fault, and it is therefore highly expedient to ascertain the condition of the breast in gravid women, in order to form an opinion as to the possibility of their nursing.

It is absolutely necessary, in addition to the measures calculated to restore and increase the strength of the infant, carefully to shield it from the influence of cold and to adopt every precaution to preserve the temperature of the body at the physiological standard. Warmth is for infants, especially for new-born infants, the indispensable condition of the continuance of life. None but the strongest children can bear any loss of temperature. The weak invariably perish if exposed to cold, and Hunter sagaciously noted the fact, and strongly objected to the practice prevalent in his day of bathing very young children in cold water for the alleged purpose of invigorating their constitution. When, therefore, a child is prematurely born, or naturally weak, it should be carefully enveloped in warm clothing, kept in a comfortable bed, and guarded in every possible manner from adverse atmospherical influences. The thermometer should be daily consulted, and hot water bottles used, if necessary, to maintain the heat of the body at a proper height.

By means of these precautions, and if required by the exhibition of aromatic and stimulating remedies, Mr. Depaul has had the good fortune of restoring, in the course of two or three weeks, children supposed not to be viable, to a normal state of development. Untiring supervision is always indispensable, as any neglect of these all-important points may entail irremediably fatal consequences.

PART IV.—MATERIA MEDICA AND THERAPEUTICS.

ART. 141.—*On the Milk Cure.*

By PHILIP KARELL, M.D., Physician to his Majesty the
Emperor of Russia.

(*Edinburgh Medical Journal*, August, 1866.)

After a learned introduction on the therapeutical cures to which milk had been devoted by many ancient and modern physicians, Dr. Karell proceeds to define what he means by the *Milk Cure*.

"If, in giving a general definition of the milk cure," he writes, "we call it a *nutritive cure*, it by no means follows that it should only be administered in diseases dependent upon a perverse nutrition. It might as well be defined as a *sedative cure*, for it is very often useful in those cases where Valsalva would in all probability have employed fasting and phlebotomy. A more exact definition, perhaps, would be, that milk, when methodically administered, is a *regulator of nutrition*. It might perhaps be urged that milk is a well-known remedy, and that every physician uses it in appropriate cases. I admit that all medical men are sufficiently well acquainted with milk as a nutritive agent, and as an antidote; but I speak from experience when I assert, that in general the cure by milk, *scrupulously administered, and in strictly measured doses*, is not sufficiently, and only very rarely, recognised as a sovereign and useful remedy.

"I have frequently," he proceeds, "during the last fifteen years, been called into consultation in cases which were thought hopeless, and in many of which I recommended the milk cure, which had never been resorted to during the whole course of the malady. I had prescribed, even before that time, the employment of milk, but without regulating its administration. It was only by degrees that I arrived at a methodical system of treatment. Experiments made by other physicians have tended to strengthen my convictions. Thus, when accompanying the late Emperor Nicholas in his travels, we arrived one day at Ishougnéff, in the centre of the Steppes, where eight regiments of cuirassiers and some other troops were encamped. An epidemic of intermittent fever

was raging at the time. I found many of the wards filled with dropsical patients, the greater number of whom had hypertrophied spleen and liver. To my great satisfaction I saw a bottle of milk at the bedside of each patient, and I learned from the senior physician, Dr. Weks, that he had given up all other modes of treatment in those special cases, having found a sovereign remedy for them in milk. Another of my colleagues, Dr. Behm, having made important observations during five years in the hospital to which I was also attached, wrote to me with regard to the malignant typhoid fever which raged in Poland and Lithuania in 1854, that he had no success in treating that epidemic until he resorted to the milk cure and the occasional use of Hungarian wine.

“My respected friend, Dr. Inozemtseff of Moscow, resorted, with the help of his assistants, during his long professional career, to the milk cure in nearly 1000 chronic cases. In his work on the *Milk Cure*, published in Moscow in 1857, he speaks of the good results which he obtained from this remedy, and affirms that its efficacy is indisputable. Nevertheless, he orders milk without defining the dose. He points out the difference between a *milk cure* and a *milk diet*, on which latter he places a patient for several years. Inozemtseff refers the good results which I have obtained to the *moderate* doses in which the milk was given. I believe that a regular mode of administration is the most rational. Milk is more easily digested when taken in small draughts and at stated intervals. If we allow milk to be taken *ad libitum*, the patient will likely soon suffer from indigestion.”

Dr. Karell relates two cases illustrative of Dr. Inozemtseff's practice, and then proceeds to a consideration of his own mode of treatment.

“I generally commence,” he writes, “the cure by employing milk *alone*, and forbidding all *other kind of nourishment*. I proceed with great caution in prescribing for the patient, three or four times daily, and at *regularly-observed intervals*, half a tumbler or a tumbler, *i.e.*, from two to six ounces, of skimmed milk. Its temperature must be made to suit the patients' taste. In winter they generally like tepid milk, heated by placing the tumbler or cup in a vessel filled with hot water. In summer they generally prefer it of the same temperature as the surrounding atmosphere. They should not gulp it all at once, but take it slowly and in small quantities, so that the saliva may get well mixed with it. Of course, the milk must be of good quality. That of town-fed cows has generally an acid reaction; that of country-fed cows is better, because its reaction is generally neutral. If the patient digest the milk well, which is proved by the *fæces* becoming solid, I gradually increase the dose. The first week is the most difficult to get over, unless the patient has strong will and firm faith in the cure. During the second week two ordinary quarts are generally administered each day. If the cure take its regular course, then the milk must be drunk four times daily—at eight in the morning, at noon, at four p.m., and at eight p.m. If the patient desire it, I change the hours, but I always insist on regular intervals being observed; for the patient will think lightly of the cure, if he be not ordered to observe some regularity while subjected to it. No confidence can be inspired, and no cure expected,

if the physician says to his patient, 'Drink milk in whatever quantities, and whenever you wish.'

"When obedient to the physician's orders, the patients complain neither of hunger or thirst, although the first doses appear very small to them. If, instead of four cups of skimmed milk, a person afflicted with a severe illness takes four large tumblerfuls of unskimmed milk, you may be sure he will not digest it, and his confidence in the remedy will be shaken at the very commencement.

"I was consulted six years ago by Mrs. B. She had been suffering for four months from chronic diarrhœa, and from vomiting. The disease was called chronic gastro-enteritis by some. The patient was emaciated, and her liver undergoing fatty degeneration. She had suffered a long time from uterine and intestinal hæmorrhages. In a consultation which I had with two experienced practitioners, I proposed the milk cure as the *refugium unicum* in this case. The two gentlemen replied that they had tried it several times, but that the lady could not digest it. I knew from what they said that the patient had partaken of milk in large doses several times daily, and had beef-tea and other food besides. We resolved to try the methodical administration of milk. I ordered skimmed milk to be given thrice (each dose containing four tablespoonfuls) during the first day, and absolutely nothing else. From that time the vomiting ceased, and after the third day the diarrhœa disappeared. The fæces acquired their normal appearance, which had not been the case for years before. At the end of the second week she could digest, without inconvenience, two bottles of milk a-day. Finally, she made a complete recovery and lived several years.

"But it must not be supposed that such an effect can generally be produced when nothing is administered except small doses of milk. I have placed patients, who were taking milk in minute quantities, also on beef-tea, white bread, and water; but I never observed the same satisfactory results after this mode of treatment. The cure never was complete when allowed anything except milk to be taken for dinner. Sometimes, when the invalid had arrived at taking from ten to twelve glasses per day, I observed a return of his illness. I had then to commence the cure anew, by prescribing milk in small doses. At the beginning of the treatment, the patient's bowels are frequently constipated, which I consider of good augury. The fæces become very hard, in consequence of the absorption of the fluid particles of the milk. This may be remedied by warm water injections, or by the use of castor-oil or rhubarb. Persons suffering from flatulence are soon relieved of it by the milk cure. If the constipation be obstinate, I order the addition every morning of a little coffee to the dose of milk, or, towards four o'clock p.m., stewed prunes or a roasted apple. If, on the other hand, diarrhœa and borborygmi be the result of this mode of cure, it proves either that the milk was too rich, or that it has been administered in too large doses. If the diarrhœa does not arise from ulceration of the intestines, it is sure to be cured by strict observance of method in this treatment.

"Feverishness is no contra-indication to its use. If the patient feel very thirsty, I allow him to drink water, or Seltzer-water. If he have

a strong desire for solid food, I allow him, at the end of the second or third week, a little stale white bread with salt, or a small piece of salt herring. At four o'clock, *i.e.*, his dinner-hour, the patient may, as in the morning, take a small quantity of stale bread. Once a day, instead of pure milk, I give him some soup made of milk and oatmeal. After continuing this treatment for five or six weeks, it may be modified (according to circumstances), by allowing only milk thrice daily, and once a steak or chop. I have found that raw meat is easiest to digest.

"The strongest opposition to treatment I have generally experienced from the patients themselves, and the cause is easily explained. If a person suffering from some chronic ailment has already been subjected to various modes of treatment without having been cured by any one of them, and if the milk cure be suggested to him, which, in his opinion, can lead to no improvement, he thinks it is the same as the verdict which declares, 'You are lost, and medicine cannot save you!' I have sometimes seen nervous patients grow seriously alarmed, and request time to reflect whether they should subject themselves to the treatment or not. Thus the patients either assert that milk is repulsive to them, or that they are unable to digest it,—this one, because he has always been troubled with his liver; another, because he smokes; while a third is afraid he will die of hunger, or pretends that he has already tried the milk cure, but was unable to continue with it, because of the disagreeable effects it produced. Others ask what purpose the milk cure can serve, when other medicines have done little, if any good. My answer then is, that milk is a food easy of digestion with every person, provided it be given with precaution, that it be of good quality, and administered in definite doses; that it is the first food of man, and that a new-born infant shows no dislike to new milk. To die of hunger, even when taking nothing but milk, is impossible, since there are people who take no other nourishment. In milk are united all the elements necessary for the nutrition of our body, and besides this substance is easily assimilated. Lastly, I add that long experience has convinced me that milk is an energetic remedy in many diseases, and that in some cases I prefer it to any other remedy. Thus I am rarely unable to persuade the patient to follow out my advice; and in the majority of cases, notably those of dropsy, I have generally had the satisfaction of receiving, in a very short while, the sincere thanks of the patient for the speedy relief he felt."

Dr. Karell relates several successful instances of the treatment, and then discusses the "indications" for and against its adoption.

"In summing up," he writes, "the phenomena always observed among the patients cured or treated by other physicians and myself, I must enumerate: An intractable state of the blood, impoverished to the utmost extent, and general dropsy; disordered innervation, assuming the forms of hysteria, or hypochondriasis; obstinate dyspepsia, neither the result of congestion of the stomach nor of ulceration, nor of cancer of that organ; in fact, catarrhal, rheumatic, and gouty affections, as also nervous maladies not the result of a *local disease*, but of quantitative and qualitative defects in the fluids; or, to speak more clearly, a constitutional disease. If the cause of the disease was apparently situated in the organs of digestion, the more strongly was I tempted to

try this cure. I have thus cured, or very much relieved, chronic irritations of the pharynx and of the œsophagus, ulcers of the stomach, and similar diseases of the digestive tract. These *gastric cases* formed the greater portion of the 200. Among these, satisfactory results were obtained in a very short time. The desponding patient became lively, the gloomy countenance brightened up, the big belly decreased in size, and, as a consequence, many other unpleasant symptoms disappeared; in a word, the patient felt quite a new man.

"And even where the seat of the malady was not always as clear as in the cases above cited; but where the disease of any organ seemed to be connected with some derangement of the digestive tract, I have invariably tried the milk cure. For I thus produce a good result, simply by regulating the diet, and by excluding indigestible articles of food. And I have thus frequently had the satisfaction to see a complete cure effected by such simple means in cases where deep-seated organic disease was suspected. My own experience and that of other physicians has shown that great improvement, and even almost a complete feeling of health, have attended this treatment, when employed in cases of organic disease of the heart, of advanced degeneration of the kidneys, &c. Taking into consideration the fact that hypertrophy of the heart and the central congestion, as well as increased bronchial secretion which result therefrom, are frequently occasioned by disorder of the abdominal circulation, I think I have found an exact indication for the milk. I have modified the milk cure according to circumstances in treating plethoric persons.

"The fatty degeneration of the arteries, and the consequent friability being so frequently one of the determining causes of apoplexy, I think we shall find an exact indication in that disease for the use of milk. Neither can I say that constitutional debility was common to all patients whom I placed under the milk cure. On the contrary, I have made persons of a florid complexion undergo the treatment—persons of muscular build and a full pulse, who are generally ordered a temperate regimen, and who, to prevent congestion and apoplexy, take bitter and saline solutions with benefit. For advanced tuberculosis we have no remedy. In cases where this disease is complicated with tubercular ulceration of the intestines, I cannot foretell very good results from the use of milk.

"Fever is no contra-indication to its use. The utmost caution, however, should be used when milk is administered in such cases. At the commencement the doses should not be increased too speedily, for the patient's stomach will not absorb more milk than it can digest.

"To sum up, I have already strongly expressed myself against the practice of extolling the milk cure as a panacea; nevertheless, I feel no hesitation in declaring that the number of cases for which I prescribed the milk cure with a great degree of confidence is very considerable, and that in these cases I could have expected no good results had I resorted to any other mode of cure."

ART. 142.—*On the Antagonism between Opium and Belladonna, and their preparations.*

(*Schmidt's Jahrbücher*, 1865.)

As early as in the year 1570, Prosper Alpin and Lebel were of opinion that a combination of belladonna with opium diminished the effect of the former. Horstius and Faber, in 1677, and Boucher of Lille, in 1766, proposed to use the two agents as antidotes to one another. In 1810, Lippi discussed this proposal, which then remained unnoticed until 1838, between which time and 1843 it was revived by Carrignan, Graves, and Angelo Poma. Since 1843 the number of recorded cases in favour of an antagonism between the preparations of opium and the alkaloids of the solanaceæ has greatly increased; and, on the other hand, many doubts have lately been expressed upon the question.

Dr. Camus, of St. Quentin (*Gaz. Hebdomadaire*, 1865), doubted the value of the cases recorded in proof of the supposed antagonism, on the ground that, in opium poisoning, atropine was scarcely ever given alone, but almost always in combination with a number of other substances; so that it was difficult to decide how much of the result might be due to the alkaloid and how much to other agencies. In order to decide the question, he instituted a series of experiments upon rabbits and sparrows, to some of which he administered by injection extract of opium, morphia, codeia, papaverin, or thebain alone; while to others he administered also, some minutes later, a determinate dose of atropine, either at once, or in successive portions, with intervals of from fifteen to twenty minutes between them. In these experiments, therefore, he had in view only the action of atropine against opium; and he reserved the action of opium against the solanaceæ as a subject for future experiment. In selecting animals for comparative experiments, Camus was especially careful that they should be of like size, age, and weight. The first series of experiments served to determine the smallest fatal dose of the several preparations of opium, and of atropine;* and each final experiment was, as a rule, preceded by one with the opiate alone, in order to obtain a standard of comparison for the results of the trials in which the administration of opium was followed by that of atropine. Dr. Camus generally administered in the later experiments the smallest fatal dose as found in the earlier ones; but it should be observed that the selected

* Dr. Camus saw death ensue after the administration of

	To Rabbits.				To Young Sparrows.			
Extr. Opii.	1 grm.	within 2h.	30m.		1 cgrm.	within 0h.	7m.	
Hydrochlor. of morph.	1	"	4	15	1	"	0 20	
Narcotin .	1	"	20	0	1	"	5 20	
Codeia .	20 cgrm.	"	20	0	4 mgrm.	"	16 0	
Papaverin .	50	"	2	7	5	"	0 8	
Thebain .	2	"	0	6	1	"	0 3	
Atropine .	1 grm.	"	0	19	2	"	2 10	

quantity of the opiate was always given at once, and the atropine sometimes at once, sometimes in successive portions.

In the first series of experiments (three with one gramme each of morphia and of sulphate of atropia), Dr. Camus could not discover any diminution of the action of the morphia; and, indeed, the two animals to which the atropia was given both died sooner than the one that was poisoned by morphia alone. The same result was obtained in two other series of trials, each of three experiments with one gramme of extract of opium, and fifty and thirty-five centigrammes of atropine. In the four following series (each of three experiments, one with the above-mentioned dose of thebain, narcotine, papaverin, or codeia alone; and the other two with the addition of atropine to twenty-five centigrammes) three of the twelve animals survived, none of the three having been treated with the opiate alone. Of all the animals experimented upon in the six series, after deducting the six poisoned by an opiate alone, only three survived; from which the author infers that there is a probability as of three to one against the existence of an antagonism between opium and belladonna. A similar number of like experiments upon sparrows produced similar results.

From these researches Dr. Camus thinks himself entitled to conclude that an antagonism between opium and belladonna does not exist in the case of either rabbits or sparrows, and expresses his belief that it does not exist as regards mankind. Against the validity of this conclusion it is remarked by Professor Winter that Dr. Camus not only administered atropine in quantities too large for the opiate that had preceded it, but that he also administered it much too soon after the opiate: thus allowing the question to be raised whether it was not the poisonous action of the atropine itself that was chiefly concerned in the result, its operation being well known to be more rapid than of opium.

The conclusions of Dr. Camus are also opposed by a case reported by Dr. W. Lubelski, of Warsaw, in which a patient poisoned by atropine recovered under the use of opium. (*Gaz. Hebd*, 1865.)

A woman who had suffered for many years from hysteria, on account of which she had taken much morphia, and had become habituated to its action, was ordered to take valerianate of atropine in a fresh attack. She was ordered to have 1 grain in sixty pills, and to take one pill twice daily. She took three pills at once, therefore 1.20 grains, and soon exhibited marked symptoms of seemingly acute atropine poisoning. Dr. Lubelski prescribed Sydenham's laudanum, of which ten drops were taken five or six times within half an hour, milk and coffee to be drunk, foot baths of ashes and flour of mustard, sinapisms to the epigastrium and extremities, and two enemata containing castor oil. The patient soon slept, and the next morning she was quite convalescent.

Dr. Lubelski considers it especially worthy of note in this case that, although the patient was accustomed to the use of opium in considerable doses, it still produced its effect. On the other hand, the circumstances partly justify the objections of Dr. Camus; since, besides opium, various other means were employed.

If we combine the whole results of these observations, it will appear that morphia and atropine are naturally opposing poisons; but that while, on the other hand, atropine diminishes or removes the hurtful

influence of morphia on the brain, without diminishing its sedative and pain-allaying power, yet on the other there is no complete antagonism between the two agents over the whole range of their action. In certain functions of the organism the antagonism is incomplete, in others it is wholly absent, and, in respect of the urinary bladder, the effects of the two poisons are alike.

ART. 143.—*An Illustration of the Antidotal Power of Tobacco in Strychnia Poisoning.*

By NORMAN CHEVERS, M.D., Senior Physician to the Calcutta Medical College Hospital.

(*Indian Annals of Medical Science*, August, 1866.)

The details of the following case appear to Dr. Chevers very clearly demonstrative of the fact that tobacco is capable of acting as an antidote in at least some cases of poisoning by Strychnia. The narrative is taken chiefly from the notes of his assistant, Mr. Chambers.

Hasse Juan, a Mahomedan girl, aged eleven years, was admitted into the First Physician's ward in the evening of the 15th March, 1865, at 5.30 P.M. with symptoms of poisoning by strychnia. From the statement of the patient, as obtained during convalescence, it appears that her husband, who is a compounder in the hospital, and with whom, although she is a mere child, she lived, had kept in his house about three grains of strychnia with other drugs, and had warned his wife to be particularly careful of the powder. On the day of her admission, the man had a quarrel with his wife and struck her. She thereupon determined to put an end to her life. At 1 o'clock she had her dinner as usual, and at about 2 P.M. she took the drug and put it into her mouth, chewing the crystals. She found the drug anything but agreeable to the taste, and spat out, as she believed, the whole of it. Immediately after this, instead of washing her mouth, she swallowed some water to get rid of the intensely bitter taste which had been left behind. Within about half an hour the unfortunate girl began to suffer from sickness and a burning sensation about her throat and stomach. This was soon followed by marked convulsive fits. She had about five strong tetanic convulsions before admission into the hospital. Immediately the husband ascertained the cause of the attack, he administered sulphate of zinc and mustard emetics. She was brought into the hospital at about 5.30 P.M., three hours after the accident. The report, on admission, was:—"Countenance anxious, skin warm, pulse quick, pupils dilated, respiration easy, heart's action considerably accelerated and fluttering against the wall of the thorax; complains of a burning and suffocating sensation about the throat and chest, extremities stretched out, hands grasping firmly the sides of the bed, legs apart, feet everted, no frequent spasms."

Shortly after admission she had a severe tetanic convulsion, involving nearly all the muscles of the body and extremities, which lasted for about one minute and a half. The chest was fixed, respiration became difficult, the body was bent backwards (opisthotonos.) This was fol-

lowed by an interval of rest and relaxation of all the muscles, but the patient seemed to be much prostrated. Slight convulsive starts and twitching of the hands and feet followed at various intervals. After this attack the patient became so sensitive that the slightest touch or movement of the bed, or a sudden noise, would cause a spasmodic jerking of the whole body. A free mustard emetic, and a cathartic enema were given upon her admission. Symptoms continued much the same as before.

Dr. Chevers saw her soon after the emetic had been taken. Pure animal charcoal mixed with melted lard was given in large quantities. Immediately after this, she began to take, after each tetanic fit, small doses of an infusion of common tobacco one drachm to the pint. The following entries are quoted from the hospital ticket:—

“First, two doses of \mathfrak{mxx} of the infusion were given in quick succession, and were followed by a third dose of \mathfrak{mxxv} . 6.50 P.M. Has a severe spasmodic jerking of the body and twitching of the hands and feet. \mathfrak{mxxx} of the infusion given.—6.52 P.M. Has another similar attack. \mathfrak{mxxx} given.—6.54 P.M. Ditto, ditto.—6.57½ P.M. Ditto, ditto.—6.58 P.M. Ditto, ditto.—7.0 P.M. Patient has a severe tetanic convulsion, nearly all the muscles of the body and extremities being involved; complains of a very distressing sensation of the chest, which is spasmodically fixed. The whole body is in a general shudder. Thumbs and feet inverted, is firmly grasping the clothes on her chest, and is piteously calling out for some one to hold her, as she feels something tearing her heart. Heart’s action greatly accelerated and fluttering against the chest. This fit lasted for about five or six minutes, and was so severe that the patient’s recovery became a question of much doubt.”

I think that, here, the entry of a full dose of the infusion has been omitted.

“7.7 P.M. Has a slight twitch of the hands and feet. \mathfrak{mxv} of the infusion given.—7.10 P.M. Ditto, ditto.—7.16 P.M. Ditto, ditto.—7.25 P.M. Has a pretty strong convulsive fit. \mathfrak{mxxv} of infusion given.—7.32 P.M. Has a slight twitch of the extremities. \mathfrak{mxx} of the infusion given.—7.45 P.M. Ditto. \mathfrak{mxij} given.—7.50 P.M. Ditto. \mathfrak{mxij} given.—8.0 P.M. Vomited once. I took this as evidence that the tobacco was acting. Matter consists of dhol and rice mixed up with some animal charcoal. Is sitting and breathing pretty easily. Pulse 144.”

The nature of the vomited matter proved clearly that the emetics given had never completely emptied the stomach,—a fact of self-evident importance.

“8.43 P.M. Has slight spasmodic starts and twitchings of extremities. \mathfrak{mxxv} of the infusion given.—9.40 P.M. Had two or three slight spasmodic startings. Bowels not yet moved. Is inclined to sleep lying on her right side. Pulse 124. Skin warm. Is breathing calmly. Can move her hands and feet without getting spasms.—10.42 P.M. Vomited once since last report. Matter consisted of undigested rice and some viscid mucus with a little tinge of blood. No recurrence of fit. Is breathing tranquilly, cannot sleep, although inclined to doze. Pulse 124.—11. P.M. Is sleeping now.—12. A.M. Vomited once since last report. Passed urine once since admission. No fit. Pulse 126. Has just fallen asleep.—1.10 A.M. Vomited once. Pulse 124.—2.30 A.M.

Vomited once again.—2.33 A.M. Had one stool since last report.—3.25 A.M. Vomited once since last report. Pulse 126. No fit.—5.35 A.M. Vomited once just now; complains of much thirst. Pulse 124. Swallowed a small quantity of water. No fit.—7.7 A.M. Vomited once a little while ago. Pupils sensitive to light.—8.30 A.M. No fit, no vomiting. Is quiet.—8.35 A.M. Vomited once a little while ago; brought up some greyish-looking matter.—12.15 P.M. Vomited twice after taking some milk and sago, retched up some undigested dholl and rice (the remains of the meal which she took before swallowing the poison.) Had one semi-solid stool.—1.30 P.M. Tried to take soup, but was obliged to reject it.”

Barley-water was ordered to be given occasionally. This was retained in the stomach for some time.

“5.15 P.M. Stomach very irritable; cannot retain any food except barley-water; vomited some carbonaceous matter. Sinapism over the epigastrium.—7.0 P.M. Is slightly feverish. \mathcal{R} effervescing draught. Stat.—9.45 P.M. Is asleep now, skin warm, pulse excited. \mathcal{R} liquor ammon. acet. Mist. \mathfrak{zj} every three hours.—6 A.M. Stomach still very irritable, cannot retain medicine; skin of ordinary temperature; no stool. Olive-oil \mathfrak{zss} . Milk q. s. every hour.

“17th March.—Much the same, pulse feeble (104.) Tongue coated but moist; no stool; pupils sensitive to light; complains of a burning sensation in the stomach; epigastric region somewhat hard and full. An enema of castor-oil and olive-oil in a pint of rice-water was given.—12 P.M. Pulse good; is sleeping now; had one stool after the enema.

“18th March.—5.30 A.M. Skin somewhat warm; pulse fair; is sleeping now; no stool.—7.30 A.M. Had one stool since last report; skin warm; pulse fair. Continue sweet-oil in \mathfrak{zj} doses every three hours; effervescing draught five doses.—6 P.M. Has not vomited to-day; took her food pretty well; no stool; tongue pretty clean; pulse 120; skin slightly above the natural temperature.—12.30 A.M. Vomited twice, matter consisted of lumps of carbonaceous substance with some bile; one stool of semi-fluid feculent and scybalous with some carbonaceous matter; took an emetic.—2 P.M. A lump of carbon and lard about the size of an olive was retched up, with about an ounce more of the same matter in small particles after the administration of the emetic; says that she feels easier; pulse 116.—5 P.M. Pulse 112; skin moderately warm; took her food pretty well; vomited once; no stool; is sleeping now. Ol. ricini \mathfrak{zj} in cinnamon-water.—Stat.

“19th March.—Improving, pulse 96; tongue clean; had one solid stool; no vomiting; retains her food; complains of a little pain in the stomach; a small bit of carbonaceous matter was brought up.—12 P.M. Had one stool; passed about four or five ounces of carbonaceous matter. Repeat the castor-oil draught.—8 P.M. Had three stools; motions contain the same blackish matter as before, about ten or twelve ounces, mixed with some semi-solid feculent matter.

“20th March.—Had one stool; motion contains some greyish-looking substance; no vomiting; takes food pretty well. Discontinue the sweet-oil; calumba mixture, thrice daily.—6 P.M. Had two stools, one of which contained some blackish matter.

"21st March.—Improving; had one stool; appetite good; tongue clean; pulse natural; no vomiting.—5.30 P.M. Had two stools, solid and of natural colour; no more vomiting.

"22nd March, 1865.—Doing well; appetite good; one stool.

"23rd March.—Much the same.

"24th March.—Discharged—well."

On this case Dr. Chevers remarks:—

"I believe that this is the first recorded instance of poisoning by the pure alkaloid of *nux vomica* occurring in a native of India. The quantity actually swallowed is of course very doubtful. Fortunately, the rash act was committed soon after a full meal of rice had been swallowed.

"The precise time which elapsed before symptoms of poisoning set in is questionable. The statement is 'within half an hour.' Tetanic spasms continued, in my presence, from about 5.45 to 9.40 o'clock P.M.

"The danger was so imminent that I considered it my duty to administer, all together, those agents which, having been recommended in strychnine poisoning, came most readily to hand. They were not incompatible with each other; and, although it may be questioned how far each aided in or retarded the recovery, I think that the *modus operandi* of two—the lard and the tobacco—was distinctly apparent.

"As much animal charcoal as could be mixed with melted lard without destroying the fluidity of the latter, was given in tablespoonfuls. The quantity of this filthy mixture swallowed was probably not less than a pound. It appears, on Dr. Taylor's authority, that the efficacy of *animal charcoal* in strychnia poisoning is uncertain. In the present case it is extremely questionable whether it had any good effect whatever. *Lard* was first recommended by Dr. W. N. Pindell, who, in the *American Journal of Medical Sciences* for October, 1855, reported that, in dogs, this fat, swallowed in large quantities, acts as an antidote to strychnia. I have also a reference to the successful use of *oil* under similar circumstances, but I cannot now find the first number of the *California Medical Journal* in which the observation appeared. . . .

"I feel confident that in this case the poison took full possession of the nervous system, the contents of the stomach never having been thoroughly evacuated as long as tetanic symptoms lasted, and that recovery was wholly due to the antidotal action of tobacco. Nothing short of death by trachealismus in tetanic eclampsia could well have been more fearful than the spasms which this poor little creature endured. I employed this antidote upon the suggestion afforded by the case related by Dr. O'Reilly, U.S.;* which is that of a man who, having swallowed six grains of strychnia, took upwards of an ounce of infusion of tobacco leaves, given in small doses at intervals, and recovered. He had previously taken an emetic which had caused copious vomiting. Much less efficacy must be attributed to the emetics in my case.

"I believe that the very large quantity of infusion of tobacco swallowed by this child, viz., at least 355 minims, or nearly five fluid

* *Medical Times and Gazette*, June 12th, 1858, p. 600, and Taylor on Poisons, *Second Edition*, page 783.

drachms of infusion of a drachm to the pint of boiling water, could not have been safely taken within three hours, and that the repeated doses could not so long have failed to produce vomiting or any other evidence of nicotism, had the power of this agent not been met and for a long time resisted by that diametrically opposite condition of the nervous system which strychnia gives rise to. This case appears to me to confirm in the strongest possible manner the general correctness of the arguments of M. Brown-Séquard, Claude Bernard, and Dr. Haldane, to the effect that strychnia does not act as a direct excitant of the nervous system, but it exaggerates excessively the reflex functions of the spinal cord, so that the slightest irritation produces tetanic convulsion. And it appears to produce the augmentation of the vitality of the spinal cord in two ways :—1st, by increasing the amount of blood in the spinal cord, by paralysing the muscular coats of the vessels which supply it ; and, 2nd, by acting in a special manner on the tissue of the cord.”

ART. 144.—*Efficacy of Persulphate of Iron as an Hæmostatic.*

By Dr. W. A. WETHERBY, New York.

(*American Journal of the Medical Sciences*, July, 1866.)

Dr. Wetherby relates the following cases illustrative of the efficacy of this drug in arresting hæmorrhage :—

“CASE 1.—On March 3 of the present year, Dr. S. T. L. Beck, of No. 40, Bond-street, New York, and myself were suddenly called in the case of Mr. M. S., the well-known musical agent and manager, upon whom one of the most skilful surgeons of this city had, a few hours previously, performed the operation of excision of the tonsils in a very satisfactory manner, and with apparent safety to the patient ; excessive hæmorrhage in such cases being of rare occurrence. On our arrival, however, the blood was flowing at a very rapid rate ; and, judging from appearances, the patient, who already had begun to show signs of syncope, had lost some forty or fifty ounces. We procured a drachm of Monsel’s salt as soon as possible, and with a moistened probang covered with the powder touched the bleeding parts.

“The effect was almost instantaneous: but lest there might be a recurrence of the difficulty, we dissolved the remainder in water, to be used as a gargle. The further management of the case was, as usual, very simple, and not a drop of blood flowed from the parts after the first application.

“CASE 2.—This case is more singular in many of its features, and may, perhaps, suggest a more extended use of this preparation than has heretofore been made.

“During the night of April 25, 1865, I was called, perhaps more to fulfil the requirements of decency than from any confidence in the efficacy of my services, to visit Dr. L. N., of this city, who was suffering from an alarming attack of hæmoptysis, which had failed to yield to the more usual remedies, as lead, opium, &c. It had been preceded for several weeks by a severe cough, profuse purulent expectoration, extreme emaciation, and, in fine, by all the symptoms incident to that slow but sure destroyer, consumption. The patient himself was aware of his condition,

and had lost all hope of recovery, but begged of me to make an effort to prolong his life for a few hours that he might dictate some last bequests to his friends abroad. Emboldened by my previous success with the article, and reflecting that all the usual legitimate preparations had failed, I sent for some *persulphate of iron*, very dry, and reduced to an impalpable powder. A small quantity of this was administered by *insufflation* into the lungs every hour during the remainder of the night and the following day. The success in arresting the hæmorrhage was *perfect*, and encouraged by this, I was induced to experiment still further in correcting the discharge of matter from the lungs. Directing its use in the same manner, though with less frequency, for some time, and assisted by the action of general remedies, I soon had the satisfaction of seeing my patient leave his bed, his room, and in a short time, his house; and now, in a few months afterwards, he is prosecuting a large and successful practice in his profession, subjecting himself to all its arduous duties with no inconvenience or ill health."

ART. 145.—*On some New Compounds of Ether: Styptic or Hæmostatic Ether.*

By BENJ. W. RICHARDSON, M.A., M.D., F.R.C.P., Senior Physician to the Royal Infirmary for Diseases of the Chest.

(*Medical Times and Gazette*, April 28, 1866.)

Dr. Richardson's researches on the production of local anæsthesia by means of ether spray have led him to invent a few new compounds of ether which cannot, he thinks, but prove useful in practice.

"HÆMOSTATIC ETHERS.—In observing the influence of the cold produced by the dispersion of absolute ether during operations, nothing has struck me," he writes, "more than the effect of the cold in immediately stopping the flow of blood. For a time, cold alone, when carried to its fullest degree, prevents all venous and capillary hæmorrhage, and even the hæmorrhage from small arterial trunks. After a time, however, as reaction returns, and the vessels relax under the influence of heat derived from the renewed circulation, there is bleeding, which, if a wound be closed too quickly, is a cause of after trouble. The observation of the immediate effects of cold led me to think that if they could be supplemented by a styptic which would spray evenly with ether, and which would take up the constricting action when the vessels commenced to relax, an important desideratum in both medical and surgical practice would be supplied.

"XYLO-STYPTIC ETHER SPRAY.—With this object before me, I requested Mr. Robbins to make for me a solution consisting of absolute ether, having a boiling point of 92° Fah., charged to saturation at a low temperature with tannin, and afterwards treated with xyloidine, a little short of saturation. The compound, made with much care, came out well. It ran easily through the spray-tube without blocking; it produced good local anæsthesia, and it possessed an agreeable odour.

"In order to test to the extreme the effects of this preparation as a styptic, I took sheep's blood, removed all the fibrine previous to co-

agulation by whipping, and then let the blood remain exposed to the air for two days to ensure partial decomposition. In this way the blood was rendered nearly as fluid as port wine, and in the most unfavourable condition for being transformed into clot. A few drachms of this blood were now placed in a saucer, the saucer having been warmed to the temperature of the body. The spray of the styptic ether was then directed upon the blood from a full-sized spray tube, and in five seconds the whole mass of blood was so thoroughly solidified that the saucer could be turned upside down without any escape of fluid. The blood, which had previously presented the odour of putrefaction, was also deodorised, and remains quite inodorous at this date—ten days after the experiment. The blood sets in a firm leathery consistence, covered on its upper surface with a fine whitish layer, with a bright vermilion colour beneath.

“These are the effects of the styptic ether on blood, the spontaneous coagulability of which has been lost, and I had the pleasure of showing these effects at the College of Physicians during a lecture on heat and cold in the treatment of disease; but these effects are trifling when compared with what takes place on blood newly drawn, and which contains fibrine. In this case the process of coagulation under the influence of the spray is the work, I had almost said, of a second.

“When this spray is directed on an open bleeding living surface, the primary effects are those produced by the cold—namely, the condensation and whitening of the tissues. If blood be flowing, it solidifies, and when the parts relax, new blood that may ooze up enters the solid blood as though it were a sponge, quickly solidifying by coagulation and stopping further flow.

“The applicability of this process for the arrest of hæmorrhage will occur to the mind of every practitioner. The substances used in the compound are innocuous, and the combined influence of the cold and the styptic are immediate, and so decisive that I can scarcely imagine any hæmorrhage they would not control. I have not had an opportunity of testing the point, but I have no doubt from the influence of the styptic on the decomposing albumen of defibrinated blood, that even in those cases of hæmorrhage where the blood is preternaturally fluid, the styptic spray would arrest the hæmorrhage entirely. Where the blood contains fibrine in a natural condition, I cannot imagine a case in which the fluid would not prevent exudation.

“The essential elements of the process are three in number :—

“1. The immediate constricting effects of cold on the blood-vessels.

“2. The chemical action of the solution on the fibrine and albumen of the blood.

“3. The extreme mechanical fineness of distribution of the fluid on the bleeding surface.

“The styptic ether can not only be applied to open wounds on the skin, but to hæmorrhage after the extraction of teeth, and, by means of a uterine tube, to hæmorrhage arising from cancerous disease of the uterus or other cause. It might also be applied to the rectum in cases of hæmorrhage from piles.

“The apparatus required for this styptic ether is mechanically the same as for ordinary ether—that is to say, my spray tube with Dr.

Clarke's hand-bellows. The tube, however, requires to be made of different metal from that ordinarily in use for local anæsthesia; and I have therefore instructed Messrs. Krohne and Sesemann to construct a special tube for the purpose.

"FERRO-STYPTIC ETHER.—I have tried other experiments with the persalts of iron, which are more or less soluble in ether, especially the perchloride; and these one and all produced, as a styptic ether, rapid coagulation of blood. Solutions of iron salts in ether are not, however, more effective than the ether I have already described; and as they destroy the tube rapidly, act upon clothing injuriously, and do not so thoroughly deodorise, I do not think they are in the main so practical.

"The styptic ether, containing xyloidine and tannin, will keep ready for use any length of time, as there is nothing in it to undergo decomposition; and as very small quantities of it are required, it will become, I trust, of standard service to the medical practitioner. It would be of great use also to surgeons on board ship, and particularly to army surgeons. In case of warfare it would be exceedingly useful on the battle field, as under the instruction of the surgeon it could be used by an orderly, so as to prevent hæmorrhage instantaneously in the case of flesh wounds. It would also form a useful addition to the medical cabinet of travellers, who by necessity are removed from the direct succour afforded by medical art."

ART. 146.—*Experimental Investigations into the Action of the Bromide of Potassium.*

By Dr. ROBERTS BARTHOLOW.

(*Cincinnati Lancet and Observer*; *American Journal of the Medical Sciences*, January, 1866.)

The author's investigations were conducted in three directions: 1st. the chemical properties; 2nd. the physiological effects; and 3d. the therapeutical uses of the salt.

The physiological effects of the salt when taken into the stomach Dr. B. sums up as follows:—

"1. It proves irritant in large doses to the mucous membrane of the stomach.

"2. It is rapidly absorbed into the blood, and may be detected soon after in the urine.

"3. It acts upon the nervous centres, producing sedation, sleep, reduces the action of the heart and arteries, lowers the temperature, and diminishes the retrograde metamorphosis of tissue."

The prolonged administration of the bromide of potassium produces, according to Dr. B., the following effects:—

"1st. It diminishes and ultimately entirely neutralizes the sexual appetite.

"2nd. It produces weakness of the muscular system.

"3rd. It is irritant to the stomach if given in considerable doses, and

"4th. It interferes with the secondary assimilation, lessening the retrograde metamorphosis of tissue."

In regard to its therapeutical uses Dr. Bartholow extols it as a *disinfectant* and *deodorizer*, as an *escharotic* in sloughing and gangrenous ulcer, phagedenic chancres, hospital gangrene, epithelioma, &c.

"The actions of the bromide of potassium physiologically considered," Dr. Bartholow states, "consist in a sedative or contra-stimulant effect upon the nervous centres, producing as secondary phenomena, sedation of the heart, anæmia of the brain, anaphrodisiac effects and diminution of the retrograde metamorphosis of tissue. It has come into use in various functional and organic nervous disorders, and in certain sexual diseases where a calmative and sedative influence is desired."

This salt Dr. Bartholow considers to be indicated as a hypnotic in states of nervous excitement without congestion of the nervous centres; in hysterical insomnia; in delirium tremens; in the insomnia of excitable business men, or, in general terms, in those forms of insomnia dependent upon excitation without increased blood supply. He has found it especially in useful irritable bladder, and the chordee of gleet.

From a careful survey of all the facts Dr. Bartholow gives the following as the *methodus medendi* of the salt in question:—

"1st. The bromide of potassium acts by absorption into the blood.

"2nd. Its effects are expended upon the nervous centres, or the cerebro-spinal axis.

"3rd. Sedation of the heart and circulation, and the various local sedative effects are secondary results of the impression made upon the nervous centres.

"4th. Its physiological effects are not very decided, and are easily modified by any local disturbance.

"5th. Its therapeutical action is still more decidedly influenced by local morbid processes.

"6th. It is indicated where a sedative to the nervous system is required—in insomnia; in great reflex excitability; nervous and spasmodic affections of the larynx and bronchi, sexual excitement, and in an irritable state of the sexual organs.

"7th. It will be effectual in the foregoing conditions, in proportion to the degree in which structural lesions are absent, or in other words, in proportion to the degree in which these morbid states are functional rather than organic."

The bromide, Dr. Bartholow asserts, possesses none of the peculiar alterant property of the iodide. Whilst this fact is true, it is undoubtedly the case that the bromide relieves the congestion of certain organs, diminishes their bulk, or, as it may be styled, produces resolution of an engorgement. Such action, apparently alterative or resolvent, is not really so. It has been exhibited mainly in certain states of the uterus and ovaries—states of hyperæmia dependent upon sexual excitement, or upon the monthly nîsus. The apparent resolvent power is, in this case, due to the sedative impression of the remedy upon the sexual organs and upon the vaso-motor nerves.

ART. 147.—*On the part which Lactic Acid and the Alkaline and Earthy Lactates perform in the Animal Economy.*

By M. BURIN DU BUISSON.

(*British and Foreign Medico-Chirurgical Review*, January, 1866.)

On this subject M. Burin du Buisson has arrived at the following conclusions:—

1. That in principle all the functions of the animal economy may be considered of two kinds—one acting by excess of acid, the other by excess of alkali, two acids only concurring to the maintenance of life—the lactic secreted in the economy, the phosphoric derived from the food.

2. That the quantity of lactic acid normally secreted in the twenty-four hours in the stomach of the adult man of the average weight of 75 kilogrammes, supposing this acid to be of 30° of Baume, and in the proportion of 1 per cent. of the gastric juice, is about 75 grammes—a quantity to which additions are made by the acid which is formed in the duodenum, jejunum, and small intestine.

3. That in health this acid is the *normal* one in the gastric juice, to the absolute exclusion of the hydrochloric, or any other acid, whether organic or mineral.

4. That the lactic acid occurs alone in the gastric apparatus, whilst in the solid and organized parts of the economy it is found conjointly with free phosphoric acid; whence it may be concluded that the presence of free lactic acid in our organs is indispensable for the production of the mixed chemico-physical phenomena essential to vital action.

5. That the young mammal has in milk the source of this acid, so indispensable to its economy and development, and in sufficient quantity; and that later, when weaned, it finds the same acid in the albuminoid matter which should form the chief portion of its nutriment, and which, like milk and feculents, is equally liable to undergo the lactic fermentation.

6. That the lactic acid procured from flesh and its juices, is identical in its properties with that contained in the gastric juice, and yet differs as regards its salts, whence we are led to believe with M. C. G. Lehmann that the free acid of muscles, and of their juices, is derived from the muscular fibre itself, under the influence of the physical functions of these organs.

7. That the free action of the stomach, besides the action which it exerts on albuminous matters, facilitates in a remarkable manner, by endosmosis, the absorption of chyme, and its passage into the alkaline blood and lymph.

8. That by the notable quantity of lactates, alkaline and earthy, with a base of soda, potassa, ammonia, magnesia, and lime, which it gives rise to, and which are found in all the active fluids of the economy—the saliva, the gastric juice, the chyle, lymph, blood, the humours of the

eye, &c., it facilitates by a special action the selection and separation of the four orders of aliments* one from the other; and that, when taken into the blood-current, these lactates become a powerful source of heat by the combustion of their acid, whilst the alkaline bases, soda, and potash, contribute to the alkaline quality of the same fluid.

9. That the bicarbonate of soda and the carbonate of magnesia, as given in small doses in aid of digestion, are inoperative until they have passed into the state of lactates of soda and magnesia.

10. And that even magnesia, when administered as a purgative, does not, according to the observation of M. Mialhe, take effect until converted in the stomach into a lactate.

ART. 148.—*On the Therapeutic Use of the Lactates.*

By J. E. PETREQUIN, Professor in the School of Medicine, Lyons.

(*British and Foreign Medico-Chirurgical Review*, Jan. 1866.)

The following is a summary of a new method of treatment of an important class of disorders:—

Professor Petrequin, unlike M. du Buisson, does not consider that one acid is alone concerned in digestion. He admits that the hydrochloric may also take part in it. The lactates to which he gives the preference are those of soda and magnesia, or rather a double salt composed of the two, which at his desire M. du Buisson succeeded in forming. It possesses the valuable property of not deliquescing like the lactate of soda, on exposure to the air, or of undergoing any change from exposure, and is fit to be given in the form of pastil, or powder. In its dried and pulverized state it is white, of extreme tenacity, with only a just perceptible odour of lactic acid, and a slight saline alkaline taste, with a faint bitter after-taste.

Before treating of the ailments for which he prescribes this salt, he dwells on the peculiar advantages which the lactic acid possesses over every other acid; and first and chief, that the stomach and intestines are able to elaborate it from the materials essential to life—namely, the aliments themselves; next, that as an organic acid, it is readily decomposable and eminently combustible. He observes that to appreciate duly these qualities, we should keep in mind the enormous quantity of the gastric juice which is indispensable for digestion, and how, in this process, the acid, or rather its alkaline salts, are decomposed, the excess of alkali being excreted by the kidneys and skin, and the acid itself, except that portion of it which is retained in the muscles, furnishing

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- * 1. Proteic, so termed (albumen, fibrin, casein, &c.)
 - 2. Amylaceous (starch, gum, sugar.)
 - 3. Fatty (oils, fats, butter, &c.)
 - 4. Mineral (earthy phosphate, alkaline chlorides, &c.)

combustible elements, carbon and hydrogen, to the blood, and becoming a source of animal heat.

The ailments connected with digestion he does not, as of old, view as originating solely in the stomach. He treats of them under buccal, gastric, and intestinal derangements.

Under the first he notices, 1st, an acid state of the saliva, co-existing with difficult digestion, alteration of the teeth, and bad breath. In cases of this kind he has prescribed with advantage from one to two or three pastils of the lactate of soda and magnesia, directing that they should be left to dissolve in the mouth, and not broken by the teeth. 2ndly, he adverts not to a vitiated quality of the saliva, but to a deficiency of its quantity, producing what may be called a dry dyspepsia from insufficient salivary secretion, and attended with laborious and imperfect digestion, a dryness of the mouth and fauces, with eructations, &c. In such cases he gives from one to two or three pastils of the same salt before each meal, with the same instructions, so as to promote the flow of saliva, and orders the repetition of them after food. He assures us that this mode of treatment has often been so beneficial and in so short a time as to have exceeded even his expectations.

The gastric digestive arrangements are less limited; he specifies four: 1. Acid dyspepsia, an aggravation of the former, often accompanied by pyrosis. For this he prescribes a dose of lactate of magnesia (thirty centig.) and a saccharine lactate of soda (twenty centig.) to be taken before a meal, presently followed by one or two pastils of the lactate of soda and magnesia. 2. Flatulent dyspepsia, with eructations, sometimes inodorous, sometimes acid, occasionally fœtid; the first two commonly connected with imperfect digestion of flatulent food, especially farinaceous. The treatment advised is much the same as the preceding, with this difference, that if the flatulency is experienced some hours after eating, the lactate should be used, not before, but after the meals. 3. Dyspepsia with gastralgia or gastrodynia. This variety of the malady is also efficiently treated by the lactates; they should be administered whenever the stomach becomes the seat of pain after taking food, or in some cases should be continued daily until the malady is relieved. 4. Natural dyspepsia, so designated by our author, in which there is neither excess of acidity, nor any of the preceding complications, but a state of atony with a feeble and altered digestion. The evacuations are fœtid, and the patient, from defective nourishment, becomes emaciated and loses strength. In such cases he considers the gastric juice to be in fault, either deficient in quantity, or altered in quality. When of the first kind, stimulation of the secreting organs is requisite, and may be effected by the alkaline lactate given in the form of powders and pastils. When of the second kind he has recourse to pepsine in conjunction with the lactates; these two being helpmates and essential to healthy digestion. He prescribes them in the form of pastils composed of ten centigrammes of the former and five of the lactate of soda and magnesia, and often with the best effects.

The functional derangements belonging to the third, the intestinal digestive process, are more obscure than the preceding—that is, physiologically considered, though their symptoms are sufficiently manifest. Their origin he attributes for most part to diet, especially of farinaceous articles, liable to accumulate and load the intestines. As the lactates in

a large dose are aperients, they are peculiarly suited for this form of ailments.*

The author takes a wide view of dyspepsia, justly remarking that, though he has treated of it locally, and that it is often merely a symptom, or a functional derangement, yet, if long continued, it may terminate in organic lesion. His advice is, that besides local, it requires general treatment, directed to the specialities of each case, with particular attention to all those circumstances likely to conduce to healthy digestion. His first indication is to reform the hygiene when depraved, to regulate the regimen when not in accordance with the precepts of science; when the meals have been irregular, to insist that they be taken at fixed hours; to direct attention to the due mastication of the food; to prohibit the use of tobacco, &c. He adds, moreover, that it is necessary to combat pathological complications by suitable means; for instance, the chloropathic, by preparations of iron and manganese; enervation by quinine; general asthenia by tonics and restoratives; and the rheumatic diathesis by mineral waters, which may be called into aid in a variety of other cases.

ART. 149.—*On Ether as a Local Application.*

By Dr. JOHN J. BLACK, of Philadelphia.

(*American Journal of the Medical Sciences*, April, 1866.)

Dr. Jules Worms has concluded from a minute examination of the deposit on the surface of aphthæ, that it consists of a fatty matter which is not to be found in any other disease of the mouth, and he infers from the solubility of the exudation in ether, that this article might prove a useful remedy for the affection. Accordingly he resorted to its application, and with the most beneficial results. This remedial agent removes, he states, "the yellowish secretion, a new epithelium promptly forms, and no trace of the superficial ulcers remains beyond slightly increased vascularity of the mucous membranes."

Prompted by this statement, Dr. Black determined to give the remedy an extended trial, and he endeavours to show, by the results, that ether locally applied is a most efficient remedy not only in aphthous ulcers but also in most of the other diseases of the mucous membrane of the mouth and adjacent parts, in which, according to the researches of Dr. Worms, the deposits are of a non-fatty nature. He writes:—

"*Aphthæ*.—We have used the application in several cases of this

* The following formula is given for these lozenges as prepared by M. Du Buisson:—

R Saccharine lactate of soda	8
Lactate of magnesia	2
Amylaceous pepsin	8
Powdered sugar	61
Mucilage of gum tragacanth	q. s.

A gramme to be prepared at a time; it should be quickly dried and kept in a dry place.

disease, and invariably found the affection to yield after a few applications, daily repeated. A camel's-hair brush was dipped in ether and applied freely over the parts; it appeared to smart a little at first, but great relief soon followed. This was certainly marked both in character and point of time, in comparison with that obtained by borax and similar preparations.

"Thrush."—In this disease above all others we have been pleased with the results of the application. The cases presented themselves in the Obstetrical wards of the Philadelphia Hospital, Blockley, which of course were fruitful in that disease—containing so many badly-nourished children. It was applied directly to the parts, as in aphthæ, with a camel's-hair brush. At first it produced or seemed to produce a slight difficulty in inspiration, which was soon relieved by a hearty cry of the infant. Of course its presence in the mouth could not have been pleasant, but in no case was it followed by an unpleasant symptom. The deposit was not immediately dissolved, but seemed to disappear gradually, and in most cases after twenty-four hours there was none whatever to be seen, and the one application completed the cure (at least the local cure). In other cases a few spots remained, and if they persisted after another twenty-four hours we repeated the application, and in every instance a cure resulted. These cases were all carefully watched, some of them for several months, and in no case was there the slightest return of the complaint. In from three to four days the mucous membranes became perfectly normal; in the interval from the disappearance of the deposit to this time it presented something of the appearance of erythematous stomatitis without the usual dryness attendant on that affection. Between twenty and thirty cases were treated in this manner, and after the disappearance of the thrush they improved wonderfully. These results tend to strengthen the idea that thrush is a local disease confined to the mouth, or at least that this part only causes inconvenience, and the constitutional troubles as it were radiate from that centre.

"Ulcer-Membranous Stomatitis."—In this disease we have had the opportunity of testing it in three cases. One supervening upon pleurisy died with extensive sloughing of the parts of the jaw involved. Another recovered without any serious trouble, and seemed to have been greatly benefited by the ether. The third case, more serious, also recovered. Here the parts were apparently in a gangrenous condition, and it only differed from true gangrene of the mouth in commencing in, and being more particularly confined to the gums, without seriously involving the cheek. The sloughs were kept well detached, the parts washed with diluted chlorinated soda, and the ether applied thoroughly morning and evening. A change for the better soon came over the parts, and the patient recovered with the loss of a portion of the alveolar border of the jaw. Of course we combined with this treatment tonics and stimulants to the fullest extent.

"The question here arises whether ether might not act beneficially locally applied to true gangrene?"

"In acute pharyngitis, the sore throat every day met with, we have found it one of the very best applications, in all its stages. We apply it with a camel's-hair brush; at first it stings for a minute or two, and then a pleasant coolness is experienced in the part, giving most marked relief, and patients almost invariably express themselves as feeling

greatly benefited. The most noticeable feature in these cases is, the rapid subsidence of the swelling and tumefaction of the parts, and which the patients never fail to notice. In chronic pharyngitis also it produces the same marked relief; and in specific and non-specific ulceration of the throat, where the patient is much troubled by the accumulations of mucus and other secretions, we have found the best plan of treatment to consist in washing out the throat well with a mop dipped in a strong alkaline solution which dissipates the mucus, &c., and then applying ether to the parts. In chronic laryngitis we have seen benefit derived from inhaling ether, not of course up to the production of anæsthesia. Here an attempt at its local application proves decidedly irritating.

"In diphtheria we have not yet had the opportunity of testing the remedy, but this is the disease in which we have always expected to derive the greatest benefit from it. While we do not consider the mere throat manifestations as the sum and substance of diphtheria, nevertheless it is rational to suppose that these exudations, when swallowed, must tend to poison the system anew, and set up exhausting diarrhœas, &c., and looking at them in this light, it is certainly of the greatest importance to get rid of them. It is also, perhaps, of not less importance to get rid of the mechanical impediment which they offer to the breathing, and also of the swelling, which is so often a serious matter. As before stated, we have not had the opportunity of testing it in this disease, but my friend, Dr. D. F. Woods, of this city, has kindly reported to me a case in which he used it, and in which he derived from it all the benefits before mentioned. He said it appeared to disperse the membrane, reduced the swelling, and altogether placed the patient in a more comfortable condition. Dr. Woods also reported to me that he had used it often in the ordinary pharyngitis, tonsillitis, &c., frequently combining nitrate of silver with it, and always with the most marked and decided benefit, surpassing in his estimation all other remedies.

"It strikes me at the moment that ether might also be useful to cleanse the teeth, gums, and tongue from sordes and such accumulations in low fevers, and might possibly produce a permanent change for the better in the secreting apparatus there situated.

"There is another trouble in which ether must prove a very valuable remedy, although we have not had the opportunity of testing it. We refer to 'herpes præputialis,' that annoying and troublesome complaint with which some persons are so much afflicted. It would also possibly prove efficacious in many skin affections, such as eczema, psoriasis, &c., the crusts having first been removed with a poultice.

"According to Professor Wood, ether has been locally applied to neuralgic pains, earache, superficial burns and scalds, and also to aid in the reduction of strangulated hernia; but in all these cases the cuticle was entire. There are doubtless many other conditions in which ether might be beneficially applied as a local agent, and which will doubtless suggest themselves in practice. There is, however, one more condition in which we must refer to its beneficial action, viz., chronic ulcers.

"The ether used in all the foregoing cases was the sulphuric ether, the 'Æther,' U. S. P.

"In regard to the manner in which ether locally applied produces its results, we remark, in the first place, that it is a local stimulus, and

appears thoroughly searching and penetrating in its action. Thus then it increases absorption, prevents or dispels congestion, and allows free osmotic action. Again, very probably it changes the nature of the local cell action, which having been turned from its normal channel may thus be driven back to its course. Again, it undoubtedly acts to a great extent chemically. It has been shown by Rokitansky and others that most of these exudative inflammations contain fat, both the mucoid exudative and the fibrinous exudative, the one running into the other, except it be checked. Now the well-known solvent power of ether over fats shows us then that it must act beneficially through its chemical properties. Other chemical changes it doubtless brings about, but of their true nature we are at present unable to determine.

“N.B.—It is well to bear in mind when using ether in any manner that great caution must always be observed to prevent the near approach of flame or heat to it, for by neglecting this point serious accidents may arise.”

ART. 150.—*On the Use of Sulphites of Lime and Soda as Remedial Agents.*

By Dr. A. FISHER.

(*American Journal of the Medical Sciences*, July, 1866.)

In a report on this subject presented to the American Medical Association, Dr. Fisher arrives at the following conclusions:—

“1. That the sulphites of soda and lime can be given to patients suffering from zymotic diseases, in large quantities, and continued for a long time without producing deleterious effects.

“2. That, in accordance with Dr. Polli’s experiments, the sulphurous acid is disengaged from the alkaline base in the system, permeating it in every part, thereby preventing fermentation or decomposition of the blood.

“3. That they do not destroy or decompose the poison in the system, but prevent its deleterious action on the blood, and consequently the tissues, until it is eliminated from the system.

“4. That the quantity prescribed should be in proportion to the malignancy of the disease; that is, the more hopeless and malignant the case, the greater should be the quantity administered in a certain time.

“5. That these remedies should not be too suddenly withheld in bad cases, but continued until the poison is carried out of the system by the emunctories.

“6. That the effect of these remedies, in well-marked cases of blood-poisoning, is imperceptible for the first few hours, but by continuing them for a day or two, the secretions become improved, and the patient is relieved of the morbid symptoms.

“7. That these remedies will not cure inflammation of an organ already established, though in cases where they are indicated, they prevent the deleterious effect of the poison until the inflammation subsides, or is relieved by appropriate remedies.

"8. That the use of these agents will not prevent the use of other remedies indicated in the particular case, unless chemically incompatible.

"9. That these remedies are generally well retained on the stomach, though the dose is large, and the sulphurous taste is disagreeable to some patients.

"10. That the more malignant and hopeless the case, the more perceptible and satisfactory will be the effects of these agents, unless the case is beyond the reach of remedies."

ART. 151.—*Medicinal Uses of Ptelea Trifoliata.*

By O. F. POTTER, M.D., St. Louis, Mo.

(*New York Medical Journal*, Dec., 1865.)

The plant known as the ptelea trifoliata, or, commonly, as the wafer-ash or wingseed, a species of so-called swamp dog-wood, is of the natural order Xanthoxylacea.

It is a shrub of from six to eight feet in height. The leaves are trifoliolate and marked with pellucid dots. The leaflets are sessile, ovate, short, acuminate, downy beneath, lateral ones inequilateral; terminal ones cunate at base, from three to four inches long by one to one and three-fourths inches wide. The flowers are polygamous, nearly one-half inch in diameter, of a greenish-white colour, and of a disagreeable odour. Stamens mostly four, with style short; fruit, a two-celled samara, nearly an inch in diameter, winged all round, nearly orbicular. It flowers in June. It is common to this country, growing more abundantly west of the Alleghanies, in shady, moist, and rocky places, generally at the edge of woods. The bark of the root possesses its peculiar medicinal properties, which it yields to boiling water, but alcohol is its best solvent. The bark, when dried, is of a light brownish-yellow colour, comes in cylindrical rolls or quills a line or two in thickness, and from one to several inches in length; is irregularly wrinkled externally, and is covered with a thin epidermis; internally it is of a yellowish-white, but becomes darker on exposure. It has a peculiar, rather aromatic smell, and a bitter, pungent and rather acrid taste, yet nothing disagreeable; the pungency is persistent, which is owing to the oil which it contains.

Dr. Potter has been using it as a tonic to follow the use of quinine in all grades of fevers, also in cases of general debility connected with gastro-enteric irritation. It is mild, unirritating, having a soothing influence on the stomach, promoting digestion. It promotes the appetite, enabling the stomach to endure suitable nourishment, and favours the early re-establishment of the digestive functions, and will be tolerated by the stomach when almost all other tonic or stimulant remedies are rejected. Dr. Potter has found it especially useful in cases of debility following a low grade of fevers, also with females after confinement, or where the menstrual functions are deranged, frequently by

sustaining the digestive and secretive functions, regulating the menstrual flow; also as a sustaining and strengthening stimulant in debility connected with or following wasting ulcers or scrofulous sores.

He has been in the habit of using it in the form of a tincture, made by putting six ounces of the bark and one-half ounce of ginger to two quarts of whisky; the dose from one to two tablespoonfuls three times a day for an adult.

He feels assured, from over ten years' experience in using it, that it will be found a most valuable and reliable remedy. It has been used occasionally by the so-called eclectic physicians, and also by the negroes of the South, who call it the scrofula root, from its usefulness in sustaining the system when debilitated by that so common disease amongst them. The old French inhabitants near St. Louis also used it many years ago as a cure for the intermittent fevers of the country, long before quinine was known. When taken for a great length of time, or in very large doses, it now and then, in some persons, occasions an erysipelatous inflammation in the surface, which, however, lasts only for a short time if its use is persevered in, and no ill effects follow it.

ART. 152.—*Phosphorus Pills.*

By Dr. RADCLIFFE, Physician to the Westminster Hospital and the National Hospital for the Paralysed and Epileptic.

(*Pharmaceutical Journal*, 1866.)

Dr. Radcliffe having tried various means of administering phosphorus, has at length succeeded in effecting this in the form of pills; and as other medical men are now ordering phosphorus in this form, we have thought it desirable to publish the formula for the information of our readers. Take of—

Phosphorus	6 grains.
Suet	600 „

Melt the suet in a stoppered bottle capable of holding twice the quantity indicated; put in the phosphorus, and when liquid, agitate the mixture until it becomes solid; roll into three-grain pills, and cover with gelatine. Each pill will contain $\frac{1}{3}$ of a grain of phosphorus.

ART. 153.—*On the Anæsthetic and Sedative Properties of Bichloride of Carbon, or Chlorocarbon.*

By Sir J. Y. SIMPSON, M.D., Professor of Medicine and Midwifery in the University of Edinburgh.

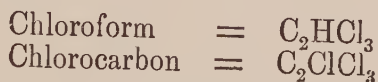
(*Medical Times and Gazette*, Dec. 16, 1865.)

As the result of personal experiments on the consequences of the inhalation of the vapours of various fluids, Sir James Simpson states that the

vapour of one of the chlorides of carbon approaches nearest in its quality and effects to chloroform as an anæsthetic agent. The fluid in question is the bichloride of carbon. He writes:—

“It was first, I believe, discovered by M. Regnault, in 1839. It has already received various appellations from various chemists, as perchloroformene, perchlorinated chloride of methyl, dichloride of carbon, carbonic chloride, tetrachloride of carbon, superchloride of carbon, perchloruretted hydrochloric ether, and perchloruretted formene (*see Gmelin's Handbook of Chemistry*, vol. vii. p. 355, and Watt's *Dictionary of Chemistry*, vol. i. p. 765).

“If it becomes, as I believe it will, for some medicinal purposes, an article of the materia medica, it will require to have a pharmaceutical name appended to it, and perhaps the designation of perchloroformene, or the shorter term chlorocarbon, may prove sufficiently distinctive. In its chemical constitution, bichloride of carbon, or chlorocarbon, is analogous to chloroform; with this difference, that the single atom of hydrogen existing in chloroform is replaced in chlorocarbon by an atom of chlorine, for the relative chemical constitution of these two bodies may be stated as follows:—



“The chlorocarbon can be made from chloroform by the action of chlorine upon that liquid; and Geuther has shown that the process may be also reversed, and chloroform produced from chlorocarbon, by treating it in an appropriate vessel with zinc and diluted sulphuric acid, and thus exposing it to the action of nascent hydrogen. The most common way hitherto adopted of forming bichloride of carbon consists in passing the vapour of bisulphide or bisulphuret of carbon, together with chlorine through a red-hot tube either made of porcelain or containing within it fragments of porcelain. There result from this process chloride of sulphur and bichloride of carbon, the latter being easily separated from the former by the action of potash.

“The bichloride of carbon, or chlorocarbon, is a transparent, colourless fluid, having an ethereal and sweetish odour, not unlike chloroform. Its specific gravity is great, being as high as 1.56, whilst chloroform is 1.49. It boils at 170° Fahr., the boiling point of chloroform being 141°. The density of its vapour is 5.33, that of chloroform being 4.2.

“Besides trying the anæsthetic effects of bichloride of carbon upon myself and others, I have used it in one or two cases of midwifery and surgery. Its primary effects are very analogous to those of chloroform, but it takes a longer time to produce the same degree of anæsthesia, and generally a longer time to recover from it. Some experiments with it upon mice and rabbits have shown this—two corresponding animals in these experiments being simultaneously exposed, under exactly similar circumstances, to the same doses of chloroform and chlorocarbon. But the depressing influence of chlorocarbon upon the heart is greater than that of chloroform; and, consequently, I believe it to be far more dangerous to employ as a general anæsthetic agent. In a case of midwifery in which it was exhibited by my friend and assistant, Dr. Black, and myself, for above an hour, with the usual anæsthetic effects,

the pulse latterly became extremely feeble and weak. In another case in which it was exhibited by Dr. Black, the patient, who had taken chloroform several times before, was unaware that the new anæsthetic was different from the old; her pulse continued steady and firm, although she is the subject of valvular disease of the heart. The surgical operations in which I have used chlorocarbon have been, the closure of a vesico-vaginal fistula, the division of the cervix uteri, the enlargement of the orifice of the vagina, and the application of potassa fusa to a large flat nævus upon the chest of a young infant. In all of these cases it answered quite well as an anæsthetic. The child did not waken up for more than an hour and a half after the employment of the caustic, which was used so as to produce a large slough. Its pulse was rapid and weak during the greatest degree of anæsthetic sleep. One of the mice exposed to its influence, and which was removed from the tumbler where the experiment upon it was made as soon as the animal fell over, breathed imperfectly for some time after being laid on the table, and then died.

“Chlorocarbon, when applied externally to the skin, acts much less as a stimulant and irritant than chloroform, and will hence, I believe, in all likelihood be found of use as a local anæsthetic in the composition of sedative liniments.

“In two cases of severe hystericalgia I have injected air loaded with the vapours of chlorocarbon into the vagina. The simplest apparatus for this purpose consists of a common enema syringe, with the nozzle introduced into the vagina, and the other extremity of the apparatus placed an inch or more down into the interior of a four-ounce phial, containing a small quantity—as an ounce or so—of the fluid whose vapour it is wished to inject through the syringe. Both patients were at once temporarily relieved from the pain. The first patient told me her relief at the first application of the anæsthetic vapour was so long that she slept during the following night far more soundly than she had done for weeks previously.

“The injection of the vapour of chlorocarbon into the rectum does not prove so irritating as the vapour of chloroform. In one case it removed speedily pains in the abdomen and back.

“Chloroform vapour, applied by sprinkling a few drops on the hand, and held near the eye, is one of the very best and most sedative collyria in some forms of conjunctivitis, ulcerations of the cornea, with photophobia, &c. I have not yet tried the vapour of chlorocarbon, but perhaps it may answer still better, as less irritant, and almost as strongly sedative.

“I have found ten or twenty drops injected subcutaneously by Dr. Wood’s syringe repeatedly relieve local pains of the walls of the chest, abdomen, &c., without being followed by the distressing nausea so frequently the result of the hypodermic injection of preparations of opium and morphia.

“Internally, I have only hitherto tried it in small doses in gastrodynia, where it has the same effect as swallowing a capsule of chloroform.

“The specimen of chlorocarbon which I have used was made by Mr. Ransford, who sent it down to Messrs. Duncan, Flockhart, and Co., of Edin-

burgh, under the idea that, by a chemical substitution, it might be converted into chloroform, and make a cheap medium for the manufacture of the latter drug. And perhaps I may be permitted here to remark that the quantity of chloroform used is now becoming very great, or possibly might be rendered greater if it could be produced at a still cheaper rate. We have two or three manufactories for chloroform in this city. The chief of these manufactories for it—that of Messrs. Duncan, Flockhart, and Co.—now make upwards of 7000 doses of chloroform every day, counting two drachms as a full dose; they thus send out nearly 2,500,000 doses a-year. Are every two million and a half full doses which are used of opium, antimony, aloes, Epsom salts, &c., attended with as little danger and as few ultimate deaths as these annual 2,500,000 doses of chloroform?”

ART. 154.—*Cases of Poisoning from Eating the Roots of the Ceanothe Crocata.*

By JOHN POPHAM, M.A., M.B., Physician to the Cork North Infirmary.

(*Dublin Quarterly Journal of Medical Science*, Feb., 1866.)

On April 15th, 1865, five boys were brought to the Cork North Infirmary, at three o'clock P.M., with symptoms of poisoning by the *ceanthe crocata*. They saw the plant growing on the banks of a stream, and mistaking it for field carrots, they all began to eat it with avidity. The effect of the poison was soon apparent. They felt a burning in the stomach and constriction of the throat, with nausea and headache, and one of the party fell down on the bank in strong convulsions. Terrified by this, the others left him, in order to get assistance; but when help arrived, he was found lying on his face, in the stream, quite dead. On being brought to the infirmary, between one and two hours after the occurrence, four out of five were relieved from the severe symptoms of the poison by emetics and other remedies, but its sequelæ, such as colic pains in the abdomen, loss of animal heat, giddiness, and depression of spirits, remained till the following day. The fifth boy, named Mulcahy, was long in a very precarious state, passing in the interval before his admission through alternations of tetanic convulsions and insensibility, with loss of speech. An emetic of sulphate of zinc was given him, and succeeded in bringing up a piece of the root; the effect was kept up by draughts of mustard and water, which produced a salutary irritation of the tongue and pharynx, rousing him from the lethargic state, as he struggled violently against their administration. His symptoms were very critical; face flushed up during his struggles; livid, when quiet; pupils dilated, and insensible; breathing slow and laboured, interrupted by constant sighing and convulsive cough; pulse eighty-four, feeble, and irregular; both the heart's sounds distinctly audible. In order to test the loss of speech, as all our efforts were unsuccessful, Dr. Popham got

his mother to speak to him, but for some time without effect, till at last a dim hazy perception of her familiar voice began to dawn upon his mind, and with a spasmodic exertion he jerked out the word "mamma." After a course of similar entreaties and shakings up, she got him to put out his tongue, but in a very hesitating and tremulous fashion. Considerable hyperæsthesia existed in the soles of the feet, the slightest tickling sufficed to rouse him from stupor; and accordingly when his somnolence waxed very profound, we availed ourselves of it, as a therapeutic agent, and he would invariably withdraw his feet with a growl of impatience. When placed in the sitting posture his head used to fall forwards, or backwards, or to the shoulder, as if the co-ordinating power of the muscles was suspended, or the polar force exhausted by the previous discharge on them of the spinal dynamic matter; but when replaced on the pillow, he tossed his head from side to side, accompanied by jactitation of the hands. As the emetic ceased to act, the stomach-pump was used by Dr. O'Sullivan, house-surgeon, and warm water was thrown in, with the effect of bringing off some imperfectly masticated flakes of the root. Strong coffee was given him, which he took willingly, stopping after each mouthful for a second or two. Other remedies, such as sinapisms to the spine and abdomen, cold affusion to head, friction and warmth to feet, and stimulants were used. Roseola was noticed on the abdomen in patches, such as Devergie describes as being occasionally observed in like cases.

Seven o'clock P.M.—He is in deep sleep, snoring loudly, and moaning, the eyelids spasmodically closed. When slapped on the cheek by the house-surgeon, he bounds up indignantly, stammering out in pitiful remonstrance, "Wisha, don't then," and covers up his head impatiently; he is immediately buried in unconsciousness again. No urine passed.

April 16th.—More conscious, but still much confused in intellect, and vacant in expression; speech returning, but he takes time to answer a question; pupils less dilated; tongue sore and swollen; pulse 84, rising to 108 on sitting up.

April 17th.—Consciousness has quite returned, but all is a blank since he ate the root up to this morning; temper irritable; tongue raw at tip and edges; he says that the quantity of the root which he swallowed was about the top joint of the little finger; the piece thrown up by the zinc emetic was about half that size. He left hospital next day quite recovered.

ART. 155.—*Remarks on the Exhibition of Charcoal-Powder.*

(*Journal of Practical Medicine and Surgery*, April, 1866.)

The researches of Duval, Brachet, and Barras, and more especially the favourable report presented to the Academy in 1849, by Patissier on a memoir by Dr. Belloc, have firmly established the reputation of poplar charcoal as the most efficacious remedy for flatulent dyspepsia. The *Journal de Chimie Médicale* in alluding to the circumstances, remarks that the best mode of exhibition of charcoal is to mix the powder

with a little pure water, but this assertion as well as the first have been recently questioned in the *Lancet* by Dr. Leared.

This gentleman contends that charcoal, prepared with the most compact timber, is an absorbent far superior to that obtained from the lighter kinds of wood. Moreover, the mode of administration generally adopted is not in his opinion the best. He argues that in order to act satisfactorily in flatulency, the charcoal should be introduced into the stomach as nearly as possible in the state in which it has been extracted from the retort. It should, therefore, be included in capsules so as to be set free in the cavity of the stomach when the gelatine is dissolved. It is objected that although swallowed in hermetically closed capsules, the charcoal must become saturated as soon as it is exposed, but Dr. Leared observes that charcoal does not really absorb moisture, but floats on the surface of the contents of the stomach, and then exercises its full power as an absorbent.

Both these views are open to discussion, and neither appears to be fully demonstrated.

A physical difficulty obviously occurs in the introduction of dry charcoal powder included in capsules into the stomach. The quantity to be exhibited in certain cases may vary from one to four spoonfuls daily, and the number of the capsules must therefore, be proportionately large; now, not only is gelatine not a nutritious substance, but it is an aperient, more injurious than useful. If, therefore, it be admitted that it is advantageous to administer the dry powder, it should simply be taken in a wafer.

ART. 156.—*On the Use of Citrate of Soda in Diabetes.*

By M. GUYOT-DANNECY.

(*Journal de Méd. de Bordeaux*; and *Bull. Génér. de Ther.*, 15 Avril, 1866.)

M. Guyot-Dannecy recommends citrate of soda, in daily doses of half a drachm to a drachm, as a remedy in diabetes. It has been shown by analysis that sugar disappears from the urine when this salt is used with the food instead of common salt. It is also known, since the researches of Wöehler, that the alkaline salts of organic acids, when given in doses too small to produce purgative effects, are absorbed, and, their acid being burned up in the respiratory process, are eliminated by the urine as carbonates. Hence citrate of soda may, without interfering with the gastric acid in the same way as alkaline carbonates, place the system under the influence of an alkaline carbonate, which is indispensable to the interstitial combustion of the glucose of the food. The efficacy of this remedy, and its superiority to the prolonged administration of bicarbonate of soda, have to be proved by clinical experience.

ART. 157.—*On the Therapeutic Uses of Oxygen.*

By M. DEMARQUAY.

(Gazette Médicale de Paris, 14 Avril, 1866.)

M. Demarquay, who has devoted much attention to the use of oxygen inhalation in medicine, says, in speaking of its therapeutic indications, that, in the early stage of phthisis, when there is no fever, and no fear of exciting local action, when the patient is becoming emaciated, and the emaciation is increased by persistent dyspepsia, oxygen may have a salutary influence in modifying the state of the constitution and sustaining the organism. Asthenia is the disease in which oxygen has been given by preference; of twenty-two patients treated by Beddoes, ten were cured, and nine relieved. But the employment of oxygen in asthenia meets with numerous contra-indications. Oxygen renders incontestible service in essential anæmia. It is specially indicated in that form of chlorosis of young girls which is characterized by obstinate anorexia; in the anæmia of convalescents, and in the anæmia, often severe, of newly-delivered females. The inhalation of oxygen is also successful in anæmia arising from hæmorrhage or from fatigue, and is also a very energetic remedy in the debility produced by prolonged suppuration; it stimulates the appetite, sustains the powers of the patient, and enables him to attain to recovery. In diabetes, under the influence of oxygen inhalation, the quantity of sugar contained in the urine is remarkably diminished. In surgery, oxygen stimulates weak and ill-conditioned ulcers, and accelerates the production of granulations in cicatrizing wounds. In senile gangrene, as long as the circulation continues in the artery of the foot, oxygen is, according to the observations of MM. Laugier, Demarquay, and Maurice Raynaud, the only remedy which in advanced cases affords a chance of recovery.

REVIEWS, BIBLIOGRAPHICAL NOTICES,

ETC.

I.—*Eighth Report of the Medical Officer of the Privy Council, 1865.* (Blue Book, 1866.)

Mr. Simon's Eighth Annual Report is of peculiar interest. In addition to questions of internal hygiene, it deals with the subject of foreign epidemics, which threaten or have reached these shores, and with the general questions of contagion in its bearings on the public health. The first portion of the Report refers to vaccination; the second, to the distribution of disease in England, and the circumstances by which it is regulated; and the third, to the epidemics which have travelled to this country from abroad—to wit, cholera and yellow fever—as well as to certain considerations on quarantine. These epidemics, and the discussions arising from them, give a special interest to the health-history of 1865, and to Mr. Simon's present Report.

I. *Vaccination*.—Of the progress of public vaccination Mr. Simon is able to report that each inspector bears witness to an improvement in its performance. "It is true," he says, "that great, very great improvement still remains to be made before the public will have nearly realized the advantages which Jenner's discovery can confer; but in the present most unsatisfactory state of the law, even a very little progress deserves mention, certifying, I think, that except for the state of the law, the progress would have been far beyond its present stage."

The belief that the cattle-plague might be controlled by vaccination exercised an extraordinary effect upon the issue of *vaccine lymph* supplied under the authority of the Lords of the Council. Mr. Simon writes:—

"Some crude speculations which had been published as to the nature of the prevailing cattle-plague, with still cruder half-promises that vaccination would prove protective against the disease, had, very naturally, filled the cattle-owners of the country with eagerness to provide the suggested security for their herds; and for a while (beginning towards the end of December) the demand for vaccine lymph was insatiable. . . . During the early part of January the applications for lymph (among which it was not generally possible to discriminate the applications of cattle-vaccinators) were so numerous as to endanger the solvency of the establishment. In the previous ten years, including periods when human small-pox had been most widely epidemic, the average number of applications in the month of January had been 932, and the highest number had not reached 1500. But in this January the applications exceeded 3000; among which only 279 could be discriminated as for veterinary purposes; and on the 18th of the

month, when this great demand culminated, the total of the one day's applications were as high as 178. Of course, whenever I learnt from the terms of an application that to vaccinate cattle was the object for which the lymph was wanted, my duty was to consider the applicant's claim as subordinate to that of persons who wanted lymph for human vaccination; and, as the lymph-supply at my disposal was not more than enough to provide for the latter purpose, I felt obliged to refuse lymph to the 279 applicants whom I could discriminate as cattle-vaccinators. There must, however, have been many other hundreds of cases where also the applicant's object was to vaccinate cattle, but where this object, being undeclared in his application, could not be discriminated and made a ground for refusal. Under these circumstances there was great reason to fear that the innumerable inconclusive trials which amateurs were making in all parts of the country as to the value of cattle-vaccination, and the consequent enormous waste of vaccine lymph, would cause such an exhaustion of the national lymph-supplies as must seriously derange human vaccination throughout the United Kingdom. Fortunately, the truth as to the uselessness of the proposed cattle-vaccination became known before that great mischief was produced: and the demand for lymph subsided as rapidly as it had risen."

Special means were taken in 1865 to determine the efficiency of the lymph issued, and Dr. Seaton reported quite satisfactorily of its quality.

II. *The Distribution of Disease in England*.—The official inquiries conducted under this head referred chiefly to the house-accommodation of the poor, the effect of sanitary improvements, and the question of injury from infected rags.

In the previous year the state of house-accommodation among the rural poor had been investigated; in 1865 the inquiry was extended to the housing of the poorer population in towns. Both investigations were conducted by Dr. Henry Julian Hunter, and his valuable reports are printed *in extenso* in the appendices to Mr. Simon's Seventh and Eighth Reports. The results of the investigation into the house-accommodation of the poor in towns are thus summed up by Mr. Simon:—

"Large as the inquiry was, and copious as are the resulting details of information, the broad results may be told in these very few words—that, neither against degrees of crowding which conduce immensely to the multiplication of disease, as well as to obvious moral evils, nor against the use of dwellings which are permanently unfit for human habitation, can local authorities in towns, except to a certain extent in some privileged places, exercise any effectual control. Resulting from (or at least attributable to) this powerlessness of the authorities, which only sometimes was supplemented by strained constructions of the law, the evils in question were found very extensively, one or both of them, in operation. Especially they were seen abounding in some of the chief places which were visited; perhaps worst in parts of the metropolis, and in Bristol, Merthyr, Newcastle, Plymouth, and Sunderland. Moreover, though in various other places, including some of considerable importance, the evils were either not found existing, or not in important degrees, it must be remembered that even in these places, or most of them, slight industrial or other local changes could rapidly and irresistibly develop high degrees of overcrowding. Adverting also to my last Annual Report, I may recall the fact, therein abundantly demonstrated, that even in small country towns and villages, where the evil least admits of excuse, overcrowding is often in glaring excess. And thus,

speaking generally, it may be said that the evils are uncontrolled in England. . . .

"It is scarcely possible for the better-off class to imagine, where duty has not given them opportunities of practically knowing, what immensity of baneful influence is included in the evils to which I advert; and it may therefore be well for me to show what in practice are the forms in which the evils present themselves. By places 'unfit for human habitation' I mean places in which by common consent even moderately healthy life is impossible to human dwellers,—places which therefore in themselves (independently of removable filth which may be about them) answer to the common conception of 'nuisances';—such, for instance, as those underground and other dwellings which permanently are almost or entirely dark and unventilable; and dwellings which are in such constructional partnership with public privies, or other depositaries of filth, that their very sources of ventilation are essentially offensive and injurious; and dwellings which have such relation to local drainage that they are habitually soaked into by water or sewage, and so forth. But beyond these instances where the dwelling would, I think, even now be deemed by common consent 'unfit for human habitation,' instances, varying in degree, are innumerable, where in small closed courts, surrounded by high buildings, and approached by narrow and perhaps winding gangways, houses of the meanest sort stand, acre after acre of them, back to back, shut from all enjoyment of light and air, with but privies and dustbins to look upon; and surely such can only be counted 'fit for human habitation,' while the standard of that humanity is low. Again, by 'overcrowded' dwellings, I mean those where dwellers are in such proportion to dwelling-space that no obtainable quantity of ventilation will keep the air of the dwelling-space free from hurtfully large accumulations of animal effluvium,—cases where the dwelling-space at its best stinks more or less with decomposing human excretions, and where, at its worst, this filthy atmosphere may (and very often does) have, working and spreading within it, the taint of some contagious fever. And as a particular class of cases, in which both evils are combined to one monstrous form of nuisance, I ought expressly to mention certain of the so-called 'tenement houses' of the poor; especially those large but ill-circumstanced houses, once perhaps wealthy inhabited, but now pauperized, and often without a span of court-yard either front or back; where in each house perhaps a dozen or more rooms are separately let to a dozen or more families—each family with but a room to itself and perhaps lodgers; and where in each house the entire large number of occupants (which even in England may be little short of a hundred) will necessarily have the use of but a single staircase, and of a privy which perhaps is placed in the cellar."

To ameliorate or remove this state of things, Mr. Simon says:—

"The powers which, in the interest of our labouring population, it is most of all desirable that local authorities should be able and willing to exercise against the evils in question, are, in my opinion, these:—

"i. To deal universally with overcrowding on the basis of its being technically a 'nuisance,' and to take, as the sole test of overcrowding, the proportion borne by number of occupants to size and ventilation of given space;

"ii. To apply to the so-called 'tenement-houses' of the poor, a system of registration and regulation akin to that which is applied to common lodging houses under the statutes of 1851 and 1853;

"iii. To enforce everywhere against the cellar-dwellings, the restrictions which, under the 67th section of the Public Health Act, 1848, are enforceable in places which are under that Act and the Local Government Act;

"iv. To exercise against premises or parts of premises which by want of access of light or air, or by dampness or through ruinous condition, are rendered unfit for human habitation, the same powers as against premises which by 'nuisances' are rendered unfit ;

"v. To acquire premises, by compulsory sale, either in order to make needful openings and clearings where ground is too closely built upon, or for other sanitary improvement."*

The inquiry into the effects of sanitary improvements, conducted by Dr. Buchanan, is not yet completed.

The question concerning the alleged occasional conveyance of morbid contagia in the various stuffs which form the staple of the rag-trade was made the subject of detailed inquiry by Dr. Bristowe. The results are thus summed up by Mr. Simon :—

"It is matter for congratulation that the results there recorded are almost entirely negative, and such, I think, as fully to establish that the rag-trade does not play any considerable part in the distribution of contagions of disease. More than this cannot, I apprehend, be maintained. When regard is had, on the one hand, to the sources of rag-supply, and on the other hand to the known properties of certain contagia, no one can suppose that paper mills do not sometimes receive rags with infective material among them ; and it would not have surprised me, if cases more or less authentic had been reported, where not only small-pox and other fevers, but also syphilitic inoculations, had on particular occasions, been ascribed to the agency of foul rags. It will be observed, however, that, except in regard of small-pox, no such accusations came under the inspector's notice ; and the instances wherein it was with some show of probability alleged that small-pox had been introduced by rags cannot be deemed to represent, in a vaccinated country, any serious amount of public danger."

III. *Foreign Epidemics*.—The year 1865 is remarkable for the extraordinary spread of pestilential diseases which occurred. First, a most deadly outbreak of epidemic cerebro-spinal meningitis, a disease hardly known even by name in this country, broke out in Northern Europe. Next, cholera again began to migrate from the East and once more extended its ravages to Europe. And while this pestilence was rapidly travelling westwards, the most dreaded of tropical plagues effected, for the first time in history, a lodgment upon our own coast. Finally, during the summer, the most malignant of bovine plagues, after more than a century's interval, revisited our herds.

Dr. J. Burdon Sanderson investigated the outbreak of epidemic cerebro-spinal meningitis in Northern Europe, and an abstract of his report, published in anticipation of Mr. Simon's report, was given in the forty-second volume of the *Half-Yearly Abstract*. The inquiries into the spread of cholera and yellow fever were conducted by Professor Parkes, Dr. Buchanan, and Mr. Radcliffe, and their reports (included in the appendix to Mr. Simon's Report) will require separate consideration.

This outbreak of cerebro-spinal meningitis was preceded by rumours of a fatal "Siberian plague" advancing westward. Upon this rumour Mr. Simon takes occasion to remark :—

"Terrible inflictions have before now come to us by that line of transit, and cholera is not the only pestilence which has thus come. Apparently it

* The first, second, and third suggestions have since been carried out in the "Sanitary Act" for 1866.—ED.

was through Russia, and perhaps as a 'Siberian plague,' that, five centuries ago, the Black Death came to England. That, according to the best authorities, the Black Death, under the name of Pali Plague, still lives, and from time to time spreads, in the western and northern parts of India, and, when last told of, was even high in the Himalaya; that, if its infection passed the hills, little story would come to us of how it filtered through the sands of nomad and other savage life; but that presently it might be on the confines of Russia, and then again suddenly of the gravest European interest;—these are considerations which, in the minds of persons who know the facts of the case, check all disposition to treat rumours of 'Siberian Plague' with indifference."

Mr. Simon believes, from information supplied to him by Dr. F. J. Brown and Dr. Clapton, that cerebro-spinal meningitis has been present for some time past in small amount in this country.

Epidemic cholera appeared only in two parts of England in 1865—at Southampton, and at Theydon-Bois, a small hamlet in Essex. Professor Parkes's report on the outbreak at Southampton we shall subsequently refer to. Mr. Radcliffe investigated the facts of the remarkable outbreak at Theydon-Bois, and the following is Mr. Simon's abstract of, and opinions on, the circumstances disclosed by his Report:—

"It is a question whether from Southampton, or in any more direct way, the morbid influence may in August or September have reached Weymouth or Portland or Dorchester. I have no proof that any such infection took place, but accidentally I am informed that a gentleman from a distance who early in August was spending a week in Weymouth, and visiting both Portland and Dorchester, contracted during that week a diarrhœa which on his return home developed to severe cholera; and in September there occurred, in the neighbourhood of London, the following events, which give peculiar interest to the question:—Mr. G. and his wife, inhabitants of Theydon-Bois, near Epping, had been lodging at Weymouth for seventeen days from the 8th September, had visited Portland on the 22nd, and Dorchester on the 23rd, and returned home on the 25th. On the evening of the 23rd Mr. G. had been seized with diarrhœa, sickness, and cramps, which continued more or less through the next day, and left him still unwell on the morning of the 25th. He, however, performed his journey to Epping with his wife. She, during the journey, began also to complain of abdominal discomfort; and this, after her return, developed, with gradually increasing diarrhœa, to cholera, of which (in its secondary fever) she eventually died on the 11th October. On the 30th September (while the last-named patient was still in collapse) one of her daughters, aged eight, was seized with cholera, and in a few hours died. That same night, a serving-lad in the house was seized with cholera, and barely escaped with his life. On the 2nd October the doctor who was attending them died of cholera after ten hours' illness. On the 3rd, another daughter of the house, aged sixteen, passed into cholera, but eventually, after some consecutive fever, recovered. On the 5th, a maid-servant got diarrhœa, which, though relieved for the time, relapsed and became choleraic on the 8th, and she, after some promise of recovery, fell into secondary fever, with which she eventually died. On the 5th also a labourer who worked on the premises, but lived apart, was taken with diarrhœa, which, passing on to cholera and collapse, killed him next day but one. On the 6th, the head of the house, the Mr. G. who had suffered at Weymouth, and had ever since had relaxed bowels, got a very acute new attack, and died after fifteen hours. On the same day his son was attacked with diarrhœa, and next day was in collapse, but rallied, and finally got well. Also on the 6th, the grandmother of the house was similarly

attacked ; and she, though she emerged from collapse, eventually died on the 14th. On the 10th a woman, living near by, whose only known connexion with the above cases was that on the 8th she had assisted in laying out the dead body of the above-mentioned labourer, was taken with choleraic purging, which soon led to collapse, and next day to death. Thus, within a fortnight, in that one little circle, eleven persons had been attacked with cholera,—mother, father, grandmother, two daughters, son, doctor, serving-lad, serving-maid, labourer, and countrywoman ; and, of these eleven, only three survived—the son, a daughter, and the serving-lad. Later, in the countrywoman's family, there was another fatal case. It cannot well be doubted but that the exciting cause of this succession of events was, in some way or other, the return of the parents from Weymouth—of the father with remains of choleraic diarrhœa still on him, of the mother with apparently the beginnings of the same complaint. But this is only part of the case, and the remainder teaches an impressive lesson. All drinking-water of the house came from a well beneath the floor of the scullery ? and into that well there was habitual soakage from the water-closet. Whether, in intimate pathology, there are any essential differences between the cholera which kills on a large scale, and the cholera which kills single victims, is hitherto so entirely unknown, that it would be idle to discuss, as a separate question, whether the G. illness, contracted at Weymouth and carried to Epping, was 'epidemic' or 'sporadic,' 'Asiatic' or 'English,' cholera ; and, as above stated, I cannot prove it to have been an offshoot of the Southampton epidemic, or otherwise of Mediterranean origin. Certain, therefore, only is this :—that, from the time when Mr. and Mrs. G. returned ailing to their home, the discharges which passed from their bowels gave an additional and peculiar taint to the already foul water-supply of their household, and that henceforth every one who drank water in the house drank water which had in it the ferment of decomposing diarrhœal matters."

Of the outbreak of yellow fever at Swansea in September, 1865, Mr. Simon has much to say of moment. He describes it as being, in one respect, an event of extreme importance :—

"That England is not insusceptible of this tropical infection," he says, "but that (at least under favouring circumstances) yellow fever can seriously damage a port-side population in England : this truth was conclusively discovered in Swansea at the cost of fully fifteen lives. Doubtless the atmospheric conditions under which the proof was given were conditions not habitual to our climate. Especially the heat was almost tropical. But no one can predict of any given year that its summer shall not produce the conditions which characterized the summer of 1865 ; nor can any one say that, if yellow fever infection should again begin to operate on our population, the mischief may not infinitely exceed those limits within which on the recent occasion it was confined. And accordingly, for the purposes of the hygienic police, the outbreak to which I refer must be deemed to have given a most impressive warning."

We cannot follow Mr. Simon in the different considerations arising out of this event as regards the public health, but the following observations on the communicability of yellow fever sum up the more important of his opinions concerning this disease :—

"I do not pretend to say that yellow fever is absolutely non-contagious in this country : non-contagious, I mean, in the sense in which typhus and small-pox are contagious : much less do I pretend to say that it is absolutely non-contagious in climates hotter than our own. This doctrine, however, even in the extremest form in which it can be stated, is not only held by many persons of high authority who have studied the disease in its trans.

atlantic strongholds, but is certainly rendered probable by facts which we have observed in Europe. . . . Quite unquestionable, however, is the evidence that the infection of yellow fever accompanies marine traffic from land to land ; and in proportion as the belief is untenable that the disease is personally contagious, in such measure the alternative must be accepted—that infectiveness is in the body of the ship. That yellow fever is a malarious rather than a truly zymotic disease, is a disease of the nature of ague rather than a disease of the nature of typhus,—that the ship which spreads infection does so irrespectively of the persons who are in it, whether they be healthy or diseased,—that the ferment of a local and impersonal infection clings to the ship from shore to shore, and breeds new malarious action in any congenial soil to which it comes,—that the exceptional and contingent power of persons to spread the disease is generally but a very scanty and transient power, not belonging particularly to the sick, but to the healthy in common with them, attaching perhaps mainly to their dress, and equally predicable of all absorbent things which the atmosphere of the ship has imbued ; this, it seems to me, is the doctrine of the yellow fever which tallies best with our present knowledge of facts. Without pretending to dogmatize theoretically on a subject which no doubt has its difficulties, I am strongly of opinion that preventive measures based upon that doctrine are, under present circumstances, amply sufficient for the practical purposes of this country. If it were, as far as practicable, provided that, during summer weather, ships which might reasonably be suspected of yellow fever infection should not come into close relation with shore or with other shipping till they, and all things in them which might carry infection, had undergone thorough disinfection, this, in my opinion, would probably suffice to prevent in future any such unfortunate occurrences as the late outbreak of yellow fever at Swansea.”

The importance of these observations in reference to the recent repeated introduction of yellow fever into Southampton Water needs no comment.

Mr. Simon's examination of the question of quarantine is of the greatest interest. Its tendency may be illustrated by one quotation, full of significance and conciseness :—

“Subject to one qualification,” he writes, “which is not an important one for the present argument, it may, I think, be accepted as certain that quarantine, conducted with extreme rigour, and with the precision of a chemical experiment, will keep cholera out of any part of Europe in which the extremely difficult conditions can be absolutely fulfilled, and thus, if I speak to the dry question of medical practice, I have no hesitation in saying that England ought to resist cholera by quarantine. On the other hand, though I cannot pretend to discuss with any kind of authority the non-medical aspects of the question, it would be mere pedantry for me to ignore that facts which are of common notoriety, and considerations which are of common sense, conflict with that medical conclusion. A quarantine which is ineffective is a mere irrational derangement of commerce ; and a quarantine, of the kind which ensures success, is more easily imagined than realized. Only in proportion as a community lives apart from the great highways and emporia of commerce, or is ready and able to treat its commerce as a subordinate political interest, only in such proportion can quarantine be made effectual for protecting it. In proportion as these circumstances are reversed, it becomes impossible to reduce to practice the paper plausibilities of quarantine. The conditions which have to be fulfilled are conditions of national seclusion ; and the fulfilment of such conditions by England would involve fundamental changes in the most established habits of the country.”

Finally, it is requisite to mention that Mr. Simon offers some observations on the recurrence and disappearance of epidemic diseases, and the probability of cosmical influences playing a part in those phenomena, which deserve attention.

II.—*Report on the Sources and Development of the present Diffusion of Cholera in Europe.* By Mr. JOHN NETTEN RADCLIFFE, Honorary Secretary of the Epidemiological Society. (*Eighth Report of Medical Officers of Privy Council, 1865.*)

This is an elaborate report prepared for the Privy Council, chiefly from official documents, on the diffusion of epidemic cholera to the close of 1865, and on the source of that diffusion. The report is divided into four sections. The first deals with the origin of the outbreak, the second with its spread, the third with the prevalence of the disorder in different localities, and the fourth contains a comparison of the recent with previous world-wide migrations of cholera. The first clearly ascertained fact of the epidemic is the arrival of two ships at Jedda in March, 1865, carrying pilgrims for Mecca from Singapore. Cholera prevailed on board these ships after they had touched the south coast of Arabia, where the malady is asserted to have been prevailing at the time. At the beginning of 1865 cholera was epidemic in the Bombay Presidency, and during that year the city of Bombay suffered from a severer outbreak than had been experienced since the "great cholera year," 1850. The facts related by Mr. Radcliffe render it in the highest degree probable that early in 1865 cholera spread along the south coast of Persia, and was conveyed to the south coast of Arabia (probably by means of coasting craft), and so reached the shores of the Red Sea; and that the two ships which first reached Jedda infected with cholera were not the sole sources of the infection of Mecca. About the 2nd of May cholera broke out violently at Mecca, and at the same time it had begun to show itself in Jedda. On the 21st of May the captain of the first ship, carrying returning pilgrims, and his wife, were attacked with cholera at Suez. On the 22nd of May a case occurred among a body of returning pilgrims at Damanhour, and on the 2nd of June a case was recognised among the inhabitants of Alexandria in communication with the returning pilgrims. The presence of cholera in the latter city was officially recognised on the 11th of June, and between that date and the 14th foul bills of health were issued to vessels leaving the port.

The following is Mr. Radcliffe's account of the spread of the epidemic:—

"On the 11th of June, 1865, the existence of epidemic cholera in Alexandria was officially recognised, and between that date and the 14th vessels leaving the port carried foul bills of health. Before the close of the month the disease had broken out in an epidemic form in Cairo, the principal towns on the Delta of the Nile (Damietta, Rosetta, Tanta, &c.), several villages on the Isthmus of Suez, and extended to Minieh, Middle Egypt. On the 20th

of the same month cholera appeared at Valetta (Malta), on the 24th at Smyrna, and on the 28th at Constantinople.

"At the beginning of July the epidemic showed itself almost contemporaneously in Jaffa and Beyrout, on the east coast of the Mediterranean, in Valencia on the West, and in Ancona on the Italian shore of the Adriatic. The town of Dardanelles would appear to have been attacked about the same time. On the 19th of July, the first case of cholera occurred at Gibraltar, and on the 22nd in Barcelona. The disease was officially recognised as present in Marseilles on the 23rd of the month;* and it appeared at San Severo in the Capitanata (South Italy) on the 25th. Towards the close of July, Cyprus was infected, and several cases of the epidemic occurred at Delos.

During the first week of August cholera broke out at Damascus, Trebizond, Kustendji, Sulina, and Tultcha (on the Danube). On the 6th the epidemic appeared at Odessa, and on or about the 11th at Aleppo. Kertch was attacked on the 17th. About the same time the disease showed itself at Madrid, Palma (Balearic Isles), and Brusa, extended (so far as can be determined by accessible data) to Reni, Brailov, and other towns of the Danubian principalities, attacked Toulon, and spread widely in the Capitanata. During the month the epidemic was probably also prevalent in the Caucasus.

"At the beginning of September the pestilence appeared at Alicante and Seville, and widened largely its area of prevalence in Spain, Southern France, and Italy. About the middle of the month it broke out in Paris and Bagdad; and on the 24th a death from the disease was registered at Southampton, the attack dating from the 17th. From the 24th to the 29th scattered cases occurred in Algiers and Prosecco (near Trieste), while an isolated and somewhat severe irruption took place at Altenburg, in Germany (twenty-four miles south of Leipzig), and a singularly virulent group of cases occurred at Theydon-Bois (Epping), Essex. On the 27th, Berditchew, in the Government of Kiev, Russian Poland, was attacked by the epidemic; and on the 28th San Giovanni a Teduccio, in the vicinity of Naples.

"Early in October cholera manifested itself in Kherson and Jerusalem, and several cases of the disease were reported at Elvas and Oporto. During the first week of the month, Naples came under the influence of the epidemic. On the 12th it appeared at Taganrog, on the sea of Azov; on the 28th at Guadeloupe, one of the Leeward Islands; on the 23rd at Mosul, on the Tigris, 216 miles N.N.W. of Bagdad; and on the 26th, in a slight form, at Filurina, in the district of Monastir, European Turkey. In the course of the month the disease spread in the Russian provinces of Kiev, Podolia, and Volhynia.

"On the 3rd of November a steam-vessel from London, viâ Havre carrying passengers, reached New York harbour with several cases of cholera on board.

"In December the malady was still prevalent in the south-western provinces of European Russia, and in some parts of France and of Saxony conterminous with the Bavarian frontier.

"The following is a summary of the dates when the epidemic first appeared in the more important localities so far as these are known:—

- June 2nd. Alexandria (officially recognised, 11th).
- „ 17th. Cairo.
- „ 20th. Malta.
- „ 24th. Smyrna.

* It is now known that the first case occurred in Marseilles on the 18th of June.—ED.

June	28th.	Constantinople.
July	1st.	Jaffa.
"	"	Beyrout.
"	"	Valencia (beginning of the month).
"	8th.	Ancona.
"	19th.	Gibraltar.
"	22nd.	Barcelona.
"	23rd.	Marseilles (officially recognised).
"	25th.	San Severo (South Italy).
Aug.	1st.	Damascus.
"	"	Sulina.
"	2nd.	Trebizond.
"	5th.	Kustendji.
"	6th.	Odessa.
"	"	Tultcha (Danube, prior to 8th).
"	10th (28th O.S.).	Borchi, district of Balta, government of Podolia (Russia).
"	11th.	Aleppo (officially declared).
"	15th. (?)	Madrid.
"	16th.	Brusa.
"	17th.	Kertch.
"	26th. (?)	Toulon.
"	"	Palma (about this date).
Sept.		Alicante } (beginning of month).
		Seville }
"	11th.	San Roque.
"	12th.	Acqui } Piedmont.
"	"	Melazzi }
"	17th.	Southampton (first death 24th).
"	18th.	Puteaux (near Paris).
"	24th.	Algiers.
"	25th.	Bagdad (earliest recorded death).
"	26th.	Theydon-Bois, Essex.
"	29th.	Berditchew (government of Kiev).
"	28th.	San Giovanni a Teduccio, near Naples.
"	29th.	Prosecco (near Trieste).
Oct.	1st.	Novomirgorod, &c., government of Kherson (Russia).
"	"	Jerusalem }
"	"	Elvas } (first fortnight).
"	"	Oporto }
"	6th.	Naples.
"	12th.	Taganrog.
"	20th.	Guadeloupe.
"	26th.	Filurina (Monastir).
Nov.	3rd.	Ship "Atalanta," from London, viâ Havre, entered New York Harbour with cases of cholera on board.

Mr. Radcliffe gives a careful account of the circumstances under which the earliest cases of cholera occurred in several of the centres of epidemic prevalence, and of the degree of its prevalence in different localities, rigidly adhering to facts without comment. Further, he gives a condensed history, of much value, of the characteristics, chronological and geographical, of former general epidemic extensions of the malady, and he makes the following comparisons (among others) of the recent with former epidemics:—

"1. For the first time in the history of epidemic cholera, Europe has been invaded by the disease from the south.

"In the outbreak of 1829-32, the epidemic spread from the north of Persia to the Russian provinces west of the Caspian Sea, and by way of the Caspian itself to Astracan. Then extending northwards along the Volga, the disease was disseminated in Central and Northern Russia, and passing westwards through the provinces of the Don Cossacks and Ekaterinoslav, and along the northern coast of the Sea of Azov and Black Sea, it attacked the south-western provinces of Russia. Next advancing through Moldavia, it reached the banks of the Danube, and ascending this river, it entered Central Europe. Southern Europe was not attacked until four years later.

"In the outbreak of 1845-48, the epidemic followed almost the same route in its invasion of Europe. Advancing from Persia through Georgia and the Caucasus, it attacked in succession Tiflis, Keylear, and Astracan. Thence traversing the provinces of the Don Cossacks and Ekaterinoslav on the southern tract of the first invasion, it radiated into Northern, Central, and Southern Russia, extended on the south to the provinces on the Danube and Turkey, and again penetrated Central Europe along the course of the river.

"In the third great European outbreak the epidemic did not advance from the east, but was developed from foci already existing in several parts of the Continent.

"In the present outbreak Southern Europe was first attacked by the epidemic; and the lines of invasion are to be traced, spreading fanlike from Alexandria to Constantinople on the east, and Southampton on the west.

"Great Britain suffered from each of the three great epidemic extensions of cholera in Europe. In every instance the disease, at the time prevailing on the west coast of the Continent, appeared first in ports on the east coast of this kingdom. In the present epidemic the disease has manifested itself first at Southampton, on the south coast, the port which maintains the closest and most rapid intercourse with Alexandria.

"During the epidemics of 1829-32 and 1845-48, Syria was invaded from the east through Koordistan and Bagdad. In the present outbreak the province was attacked in the first instance through the coast towns in regular communication with Alexandria, and subsequently by way of Damascus, shortly after the return of the Mecca caravan.

"2. In the epidemic of 1829-32 Astracan was attacked on the 20th July, 1830; Hamburg in September, 1831; Sunderland on the 26th October the same year, and New York in June, 1832.

"The epidemic occupied nearly fourteen months, from the time of its entrance into Europe, in traversing the continent; fifteen months in reaching Great Britain; and two years, less one month, in arriving on the North American coast.

"In the epidemic of 1845-48, the disease reached Astracan in June, 1847; Hamburg in September, 1848 (crossing the Continent of Europe in nearly the same period of time as in the former epidemic); England in the same month (Horsleydown, 22nd September, 1848); and New Orleans on the 2nd December of the same year, nineteen months after the appearance of the epidemic on the eastern border of Europe.

"In the present epidemic the first case of cholera was recognised in Alexandria on the 2nd June, 1865. Eighteen days afterwards (20th June) the disease appeared in Malta; twenty-two days afterwards (24th June) in Smyrna; and twenty-six days afterwards (28th June) in Constantinople. In five weeks (8th July) Ancona was attacked, and in seven weeks (23rd July,

officially recognised) Marseilles. On the 17th September the first case of cholera occurred in Southampton; and on the 20th October the epidemic broke out in Guadeloupe.

“In less than five months from the appearance of cholera in Alexandria, and about six months from its outbreak at Mecca, the disease had spread from the coasts of the Euxine to the western hemisphere, a rapidity of progress unparalleled in previous epidemics.”

In an appendix, Mr. Radcliffe discusses the effects of Hindoo pilgrimages in fostering and disseminating cholera; and gives a well-nigh exhaustive account of the pilgrimage to Mecca.

III.—*On the Outbreak of Cholera in and about Southampton in September and October, 1865.* By Professor PARKES, M.D., F.R.S. (*Eighth Report of Medical Officer of Privy Council, 1865.*)

This report is of singular interest. It is an exhaustive examination of the circumstances under which cholera appeared in Southampton, in 1865—the first outbreak of the disease in this country during the recent epidemic. Professor Parkes describes in detail the different local outbreaks, and he then proceeds to discuss the diagnosis and origin of the disease, and to recount the preventive and curative measures adopted. He examines the most important question, that of origin, in reference to importation from the Mediterranean, the agency of wind, of meteorological conditions, or a peculiar epidemic constitution, and he sums up the result of his inquiry thus:—

“But what now is the conclusion of this inquiry? If I have exhausted all the reasonable or possible modes of origin, and I cannot see there is more to be considered, the result is this:—the spontaneous development from usual meteorological, with or without bad local sanitary conditions, must be rejected; the origin by an unknown epidemic influence, alone or coinciding with local conditions, presents formidable difficulties, even if we cannot quite reject it. The origin by importation is deficient in precision of evidence.

“Still, both from its own evidence, and from the impossibility of indicating another satisfactory mode of origin, the importation seems to me by far the most likely cause. There is at any rate a coincidence, and one which could scarcely be accidental. In only one part in all England did vessels arrive having had cholera deaths on board shortly before, and in reality having cholera on board when they entered the port. Then in that port there occurred shortly afterwards other cases of cholera, and if these were scattered, so also must have been the seeds of the disease if they were carried by the persons suffering from cholera or slight cholera, landing from the vessels, and dispersing to their homes in and round Southampton, or if the germs of the disease in the atmosphere drifted from the ship, and here and there found the necessary conditions for existence and propagation.

“At any rate, this is really the only tangible point, as far as I can see, and it has the additional chance of being true, that it is in accordance with the fact that outbreaks of cholera have so frequently happened in towns after the arrival of persons from affected places. Those who know

the history of cholera are aware how many instances have proved the truth of the rule.

“The outbreak in Southampton resembled also many other outbreaks in its main features. Even admitting importation, it has often proved most difficult to trace the connexion of the early cases, and often also the cases which have occurred some time after the introduction.

“This difficulty is certainly much greater in cholera than in the cases of other specific and contagious diseases. Admitting that there is occasional difficulty even in small-pox, and still more in scarlet fever, and still more probably in typhoid fever, yet the ordinary rules of contagion are observed with much greater ease than in the case of cholera. The susceptible persons nearest to those affected or having intercourse with them, or being in some way most exposed to the putrifying portions of the body coming from the sick, suffer first; then the disease spreads to others, but still can be traced until its steps cross each other too often to remain distinct.

“But in cholera it is only in a few cases that this can be traced, and this difficulty, in fact, has been felt by almost all the observers who have inquired into the origin of cholera, both in India and Europe. Still, though as in London in 1848, and in Southampton in 1865, it seems impossible to indicate the exact entrance, or the connexion of the early cases with some prior case, the apparently certain knowledge we now have that the discharges will propagate the disease, and that these discharges will retain that power for a considerable period, may explain some of the apparently capricious and unaccountable outbreaks of cholera.

“It may be, however, that greater care, and an earlier and more stringent inquiry into the first cases would clear this up, and the obscurities may have their origin in the inquirer, rather than in the problem under examination, though I cannot but believe there is something in the spread of cholera which no explanation hitherto suggested has yet touched.”

IV.—*On the Outbreak of Yellow Fever at Swansea.* By Dr. GEORGE BUCHANAN. (*Eighth Report of Medical Officer of Privy Council, 1865.*)

On the 9th September, 1865, a ship, the *Hecla*, which had sailed from Cuba on the 26th July with cases of yellow fever on board, entered Swansea Harbour. When she came to her moorings, one of her seamen was dying and two were recovering from the disease. She landed her sick immediately, commenced to discharge her cargo, and remained stationary until the 28th. Meanwhile, from the 15th September, six days after her arrival, to the 4th October, six days after her removal from her moorings, twenty of the inhabitants of Swansea were attacked with yellow fever (a phenomenon unparalleled in the experience of England), besides others affected less definitely or more mildly. The people attacked were not scattered indiscriminately over the whole town, but only in definite local relations to the ship. And at Llanelly, three of the crew of a small vessel which had laid two days alongside the *Hecla* at Swansea, also fell sick of the fever. During the outbreak an almost tropical heat, a rare circumstance, prevailed at Swansea. The circumstances attending the dissemination of the disease in Swansea are thus commented upon by Dr. Buchanan:—

"As to the connexion of the disease with the *Hecla*, the evidence appears conclusive, (a) From the fact that there had been for months no other vessel in the harbour that had had any yellow fever on board: (b) From a consideration of dates. A vessel which has acknowledgedly lost part of its crew from yellow fever, and which lands a man on her arrival to die of that disease in a few hours, enters Swansea on September 9th, and remains there till September 28th. From September 15th, six days after her arrival, to October 4th, six days after her departure, cases of a disease previously unknown at the port break out, with the symptoms and fatality that mark it for yellow fever. The vessel leaves the dock on September 28th, and takes up a distant position near the harbour mouth, and from October 4th to October 23rd, the date of the report, there is no fresh case; (3) The locality where the disease occurred again connects it with the *Hecla*. In a town of 30,000 people, some 18 cottages are scattered on a little low-lying island, to which the vessel importing yellow fever comes, and on which she discharges her cargo. Of the 22 cases of the fever (excluding doubtful cases of it) that subsequently break out, 11 occur in persons resident in the little island, 5 in persons who, living elsewhere in the town, have their daily work on the island, 3 in men occupied about shipping in the North Dock, and only two cases occur among the whole population of the large town who had no direct connexion with the island. But even these two cases occur in persons living within 150 yards of the ship, across the dock, and living in the next house but one to the cottage where the man died who was taken from the vessel on her arrival."* "Granting the original connexion of the disease with the *Hecla*, it must also be granted (see tabular statement of cases) that in the great majority of instances the disease occurred in the individual without communication with any previous sufferer. But how does the evidence stand about its having been communicated in any case or cases by personal contagion? On the one side in favour of such contagion are to be alleged the facts (a) that in one house on the island five persons of one family (Mahoney) were attacked in succession with more or less positive yellow fever, and that Mrs. Williams had had the opportunity of personal contagion from the corpse of a neighbour; (b) that in another house, at a distance from the island, where a man (Colwell) died of the fever, another man (Jones) was also attacked; (c) that two cases (Hickey) originated in the immediate proximity of the house where the originally imported case (Saunders) died; (d) that one of the medical men, Dr. Griffiths, had an attack simulating yellow fever after attendance on a case. But to each of these considerations there is a drawback that greatly destroys their apparent value, for (a) each of the Mahoneys and Mrs. Williams were exposed by residence near the ship to the same direct infection from her; (b) Jones, as well as his fellow-lodger, had worked on the island at such an interval before as would just make the period of incubation observed in other cases; (c) the Hickeys lived within a short distance from the ship, though away from the island, or if they did get the disease from Saunders, being the only people who received the disease from another, the fact that Saunders came direct from the *Hecla*, and might have brought some of her atmosphere with him, separates this case from all others of apparent personal contagion; and lastly (d) Dr. Griffiths's case is weak, inasmuch as his symptoms were, in some essential features, unlike those of

* Of the seven doubtful cases five resided on the island; one was occupied in a vessel alongside the *Hecla*; and the remaining one (one of the most questionable of all the doubtful cases) alone had nothing to do with the docks or island.

slight cases of yellow fever, and also inasmuch as there was with him no exposure till two days before his attack, a period below the incubation time of the disease as observed in Swansea.

“ On the other side, the evidence tending to negative personal contagion is about as strong as such evidence can by its nature ever be. Persons exposed to the fever-producing influences about the docks lay sick of yellow fever in various parts of the town ; Norman at Clifton-row ; Bowen at Fynnon-street ; Margaret Williams at Gower-place ; Jesse at Greenfield-street ; Thomas at Sketty ; Lilley at Clifton-hill ; Colwell and Jones (for it is fair to quote them on this side the question) at Lower Rodney-street ; Wilkins at Bethesda-terrace ; Mrs. Wilson at Mansell-street ; and Nathaniel Williams at Powell-street. Moreover, Slocum died at Llanelly, and Stapleton was sick at Frampton-on Severn, and no extension of the fever occurred at either of those places. Thus that there were twelve centres from whence the disease, if it had been communicable from person to person, had the opportunity of spreading, and many of these localities were perfectly adapted for the spread of contagious diseases ; yet in no single instance out of all these did any person (whose business did not lead them to the infected neighbourhood of the docks) get yellow fever or any disease at all simulating it. The conclusion then appears indisputable that if the fever was communicable at all by personal contagion, it was so only in an extremely feeble degree. If it had behaved like any of the more contagious fevers, such as small-pox, measles, typhus, or relapsing fever, it is quite certain that no such account as this could be given. The contrary belief, that infection was received by each person severally, direct from the *Hecla*, is further rendered very strong by the fact that after the removal of the *Hecla* (and allowing for the incubation period in persons already infected) no fresh attack whatever occurred on the island or elsewhere, although the same climatic conditions persisted for some time after.”

Dr. Buchanan's report of this remarkable outbreak leaves nothing to be desired.

V.—*Statistical, Sanitary, and Medical Reports : Army Medical Department.* Vol. VI. for the Year 1864. (Blue Book.)

The Sixth Annual Report of the Army Medical Department, just published, refers to the health of the forces in 1864. It presents an elaborate account of the sickness, mortality, and invaliding of the troops, and of the sanitary condition of the principal stations at home and abroad, during that period ; and is enriched with numerous valuable medical and surgical reports. In addition, anticipating the report of 1865, it contains accounts of the outbreaks of cholera in Malta and Gibraltar during the latter year, as well as on board the ship *Renown*, on her passage from Gibraltar to the Cape of Good Hope. These reports (that respecting Malta being written by Surgeon Leith Adams and Assistant-Surgeon F. H. Welsh ; respecting Gibraltar, by Deputy Inspector-General Dr. Rutherford ; and in reference to the ship *Renown*, by Deputy Inspector-General Dr. Lawson) give a special and immediate interest to the present volume, from the important light which they throw upon the propagation of the prevailing epidemic of cholera, and upon the practical questions submitted to, and the decisions arrived at

by, the International Sanitary Conference. By hastening the publication of these reports the Army Medical Department has done a great service to the public, and a deserved justice to the reporters.

The health of the total force, abroad as well as at home, was higher in 1864 (as measured by the sickness and mortality) than the average of the four preceding years. The annual ratio of sickness per 1000 strength was 1119, and of deaths 16·13; while during the four years 1860-63 the annual ratio of sickness per 1000 strength had been 1216, and of deaths 17·57.

Although the returns for the total force show a higher state of health, in five of the twelve chief commands the sickness or mortality during 1864 was in excess of the average of the four previous years. In the United Kingdom the admissions were 44 per 1000 under, and the deaths 75 per 1000 above, the average of the four years 1860-63. The difference, Dr. Balfour observes, was "most marked in the admissions for miasmatic diseases, and in the mortality by diseases of the circulatory system. This increase in the ratio of deaths in 1864 has not, however, been confined to the army; as the Registrar-General's returns show the mortality of the general population of England and Wales to have been 1·71 per 1000 in excess of the average of the ten preceding years, and to have been higher than in any year since 1849, when cholera prevailed as an epidemic throughout the country."

A formidable increase of the mortality in British America—the annual ratio per 1000 strength being 23·90 in 1864, as compared with 9·18 in the four preceding years—was owing chiefly to a serious outbreak of yellow fever among the troops stationed at Bermuda. A detailed and peculiarly interesting and valuable report of this outbreak, by Deputy Inspector-General Barrow, was published in the Fifth Annual Report of the Department.

An excess of deaths in the West Indies arose from an unusual mortality in the Windward and Leeward Command, particularly in British Guiana.

In Ceylon the prevalence and fatality of continued fevers, dysentery, diarrhoea, spasmodic cholera, and respiratory disorders raised the mortality above the average of the preceding fourteen years; and the war in New Zealand caused an excessive sickness and death-rate in the Australasian Command.

The Report of the Sanitary Branch of the Department shows a steady and continuous improvement in the hygienic condition of the stations occupied by the army at home and abroad. It also describes the numerous defects in many stations which still require to be remedied. In addition to the reports on cholera already mentioned, and which are included in this section of the Department's Report, it contains an instructive medico-topographical Report on Japan, by Surgeon G. P. M. Woodward, 2nd Battalion 20th Regiment; a narrative of the outbreak of cholera on board the steamship *England*, by Deputy Inspector-General Barrow; Professor Parkes's accustomed valuable annual review of the progress of hygiene; and others papers of interest.

Among the incidental subjects of the Sanitary Report, an ingenious field hammock, lately designed by Captain M'Gwire, of the Royal Regiment, and tested practically last year at the Curragh Camp, deserves

particular mention. In a report on this article of field equipment, Staff Surgeon-Major Dr. Ord Mackenzie concludes that the advantages of the hammock, in a sanitary point of view, are simple and clear. "1. A hammock," he writes, "is under any circumstances more comfortable than the ground, wet or dry. 2. The soldier carries his bed with him, and can pitch it anywhere, under cover or not. 3. The bed can be pitched in a few minutes. 4. The occupant is always off the ground. 5. The equal distribution in either case economizes space, and gives each man a clear berth. 6. The heads of the men are considerably raised above the draught from under the curtain of the tent." The hammock is easily carried by the soldier himself as part of his field equipment, the extra weight over the ordinary field blanket being under three pounds; and Dr. Mackenzie believes that its use is practicable, simple, and likely to be most serviceable.

The report of the Medical branch of the Department contains many interesting medical and surgical papers, amongst which an elaborate continuation of Deputy Inspector-General Lawson's observations on so-called "pandemic waves" has special prominence. Amongst the contributors are Staff Surgeon Dr. P. Davidson (who discusses the Cestoid Worms), Dr. Stewart (surgeon 18th Regiment), Dr. Belcher, Professor Longmore, Dr. Fyffe, Inspector-General Dr. Muir, C.B. (who gives a sketch of Sherman's march), and others.

VI.—*Report of the Cholera Epidemic of 1865 in the Maltese Islands, together with an Introduction and an Epitome of the Cholera Epidemics that have previously ravaged them.*
By Surgeon A. LEITH ADAMS, and Assistant-Surgeon F. H. Welch, 1st Battalion, 22nd Regiment. (*Statistical, Sanitary, and Medical Reports of the Army Medical Department.* Vol. VI., 1866. (Blue Book.)

This is an elaborate and most valuable account of the cholera epidemic of 1865 in the Maltese islands. It is preceded by an authentic history of previous outbreaks of the epidemic in these islands, which is not the least valuable part of the report, as no connected history of the earlier outbreaks was accessible. The chief defect of the report is the included general history of the diffusion of cholera in the Mediterranean in 1865. It would have been well if this had been omitted, as the writers had not the means of obtaining an accurate knowledge of facts and dates, and consequently this portion of their work contains many errors. In all, however, that came under their own immediate knowledge and inquiry their report is of the highest importance, and reflects the greatest credit upon the energy and precision with which they conducted their investigations.

We shall confine our attention to two points of this report, the first relating to the introduction of cholera into the islands, and the second to the relation of the diarrhoea prevalent during the epidemic to the fully developed disease.

Towards the end of May, and before there was any knowledge of cholera having appeared at Alexandria, numerous pilgrims returning from Mecca had landed at Malta without the imposition of quarantine. About the same time the alarm beginning to spread in Egypt of cholera appearing in the track of the returning pilgrims, many Maltese returned home from Alexandria. A quarantine was not established against the Egyptian city until the 14th June, cholera having been officially declared to exist in Alexandria on the 11th, and the first vessel which arrived at Valetta after the establishment of quarantine had lost at least one of its crew from the disease. The first clearly noted case of cholera in Malta occurred in the so-called Plague Hospital, near the quarantine station, on the 20th June. But the reporters, from an attentive consideration of the whole facts, are of opinion that Malta was infected by the choleraic poison prior to the commencement of the quarantine against Alexandria on the 14th June, and that the poison came first in the tract of the pilgrims and earlier fugitives.

The facts relating to the appearance of cholera in the island of Gozo are very precise. A sailor, who had been serving on board a small vessel in the harbour of Valetta, returned to his home on the island, with all the symptoms of cholera, on the 21st July. He was nursed by his two sisters and two other women. All of these were attacked on the 24th, and on the 25th another female attendant. From this last-named date and these cases the disease spread among the population of the island.

Prior to the onset of the epidemic there was no prevalence of bowel-complaint, either among the military or civil populations. Of the diarrhœa which existed during the epidemic, the reporters describe three varieties in the following terms :—

“1st. The common ordinary summer diarrhœa, characterized by pains in the stomach, foul coated tongue, and numerous bilious stools, and very tractable in its nature. This was more prevalent than ordinary, and, no doubt, often caused by the irregular habits and drunkenness so rife among the soldiers during the epidemic. That it was also wide-spread among the civil population, the narrow streets and lanes outside the towns and casals fully attested.* It was ascribed by the native practitioners to the more than ordinary consumption of fruit by the poorer classes, bought by them at a very cheap rate, owing to the usual consumers (the better classes) avoiding it most superstitiously as likely to produce the cholera, and substituting it by animal food.

“2ndly. There was a diarrhœa not previously existing, characterized by painless watery purging, and often accompanied by vomiting of same character, clean or white furred tongue, depression of the countenance, dark rim under the eyes, and exhaustion. It could be found in every degree of intensity, and, when severe, was classed under ‘choleraic diarrhœa.’ Although intractable, it evinced no tendency to pass beyond a certain point, or assume a more malignant form, and was best treated on cholera principles. It was very frequent at the height and during the decline of the

* The houses of the poorer class in Malta are often without any convenience whatever; and, consequently, while the male portion of the family “ease themselves” outside the villages and towns, the younger members make use of the street itself.

epidemic. The 29th and 84th Regiments remarkably well exemplified this form.

"3rdly. There was the diarrhœa, an intensification of the second kind, and so completely intractable, that in sixty-one cases, where every possible attempt was made to check it, in none did it succeed, but was invariably followed by full development of cholera,—in fact, it was the diarrhœa stage.

"Among some medical officers, plans were put in action for the detection and checking of the bowel-flux by means of astringent mixtures and pills, easily attainable by the men. Room to room visitation by an intelligent corporal or hospital orderly was adopted in the 1st Battalion 22nd Foot, and any man requiring more than two doses was sent at once to hospital. By others but little stress was laid upon this symptom, and more on the general appearance of the person, or the presence of *malaise*. However, the objects aimed at by all were to check minor bowel-complaints, to get any suspicious case as quickly as possible under surveillance, and by attention to the daily numerical applications for medicine, to have an indication of the tendency to health or otherwise of the men.

"It must be clearly stated, that during no period of the epidemic was the 'premonitory diarrhœa tending towards cholera, but easily checked,' met with. The second variety showed no tendency to pass beyond a certain point if not stopped. Its severe form, the third variety, was clearly a stage of the disease; and it may be fairly questioned whether a single case was prevented developing itself into cholera by treatment directed towards suppression of the intestinal flux. It was well to get rid of the first variety, on account of the supposed 'predisposition to imbibe the virus shown by persons labouring under bowel-complaints.'"

The account of the propagation of the disease throughout the islands, its symptoms, and the result of treatment, is full, and of great value.

VII.—*Lectures on Epilepsy, Pain, Paralysis, and certain other Disorders of the Nervous System.* By CHARLES BLAND RADCLIFFE, M.D., F.R.C.P. Lond., Physician to the Westminster Hospital, and to the National Hospital for the Paralysed and Epileptic. 8vo, pp. 280. Philadelphia: Lindsay and Makiston. 1866.

This is an American reprint of Dr. Radcliffe's well-known lectures delivered before the Royal College of Physicians. We gather from it that English authors honoured with transatlantic fame have more to be thankful for in the republication of their works than is commonly known. One of the American reviewers of Dr. Radcliffe's work, after complimenting the author highly upon his style, expressed a regret that it should be disfigured by the "obsolete orthography of Webster." This defect, however, in the reprint before us, has been courteously removed by the American compositors, and we may, therefore, hope that this edition will prove as satisfactory to our transatlantic brethren as the English edition has to the profession at home.

VIII.—REPORTS OF THE INTERNATIONAL SANITARY CONFERENCE.—*Rapport sur les questions du Programme relatives à l'Origine, à l'Endémicité, à la Transmissibilité, et à la Propagation du Choléra.*

Rapport sur les Mesures à prendre en Orient pour prévenir de nouvelles invasions du Choléra en Europe.

Rapport sur les Mesures d'Hygiène à prendre pour la préservation contre le Choléra Asiatique.

La Désinfection appliquée au Choléra. Par le D. MÜHLIG. Travail revu et approuvé par la Commission (Appendice au rapport sur les mesures hygiéniques).

Rapport sur les Mesures quaranténaires applicables aux provenances Cholériques. Constantinople. Imprimerie Centrale, 1866.

The International Sanitary Conference arose out of a suggestion of the French Government late in 1865. Epidemic cholera at that time had again found its way into several parts of Europe, travelling thither by a new route—a route which, on the first aspect, threatened greater future danger to Western nations than the lines of direction which had been previously followed by the disease in its migrations westward. The French Government addressed a note to the different Powers of Europe, and, laying special stress upon the foregoing fact and inference, suggested that a Conference should be held by the European powers to discuss the question whether it would be practicable to devise measures to prevent future migrations of cholera to Europe, and generally for the more effective limitation of the disease under any circumstances. The suggestion was readily responded to; and as it was assumed from the facts of extension of the recent epidemic that the execution of any measures devised by the Conference for the exclusion of cholera from Europe would probably fall chiefly upon Turkey, it was determined, out of due courtesy to the Porte, that the Conference should be held in Constantinople. Accordingly medical, and in the case of the greater Powers, political delegates from the following States assembled in that city early in the present year, namely—Austria, Belgium, Denmark, Spain, the Papal States, France, Great Britain, Greece, Italy, Holland, Persia, Portugal, Prussia, Russia, Sweden, and Turkey and Egypt. The discussions of the Conference occupied several months; and the results, with one exception, are published in the reports named above. Another report of the Conference, devoted to a historical sketch of the epidemic of 1865, has yet to appear.

Either from the impracticability of the subject, or from a want of soundness in the original scheme, the labours of the Conference have not ended so satisfactorily as was desirable. They have added nothing to our previous knowledge of the disease, or of our means of preserva-

tion from it. Almost all their practicable suggestions have been anticipated; and for the rest, novelty does not beget admiration when conjoined with impracticability. Certainly the Conference has brought together all that is best known of the etiology, mode of diffusion, and hygiene of cholera, and set forth this knowledge in a very compendious, although not always trustworthy, form. The results thus obtained, it may be surmised, will exercise a beneficial effect on the different states of Europe by bringing their practices in reference to the prevention of cholera more into a common accord. But whether this somewhat lame conclusion is to be deemed a sufficient return for so costly an investigation may be questioned.

The Conference holds that India is the birth-place of cholera, that the disease is endemic there, and that it is only known beyond the borders of that country through its propagation by man. It maintains that the facts showing the transmissibility of cholera, and its diffusion by those sick of the malady, are absolute. Whilst holding that the sick of the disease, whether in its diarrhœal or collapsed form, are the principal agents in its dissemination, it further concludes that under certain circumstances various stuffs, particularly such as may have been saturated with the discharges of the sick, may contribute to its spread. In addition the Conference holds that the dejections of the sick from cholera contain the active agent in the propagation of the disease, and that this propagation is effected mainly through the medium of the soil, of water, and by atmospheric dispersion. Upon these leading conclusions the principal recommendations of the Conference are founded.

With regard to the endemicity of the cholera in India, they suggest that a systematic inquiry should be made into the causes of this endemicity; also that the sanitary measures already in process of being carried out in the different Presidencies should be extended. Particularly stress is laid upon the importance of the sanitary regulation of the Hindoo pilgrimages. The immense accumulation of persons which takes place during the pilgrimages which are made to the numerous shrines scattered throughout India is regarded as the most fertile cause of the maintenance and propagation of epidemic cholera. Similarly the great Mohammedan pilgrimages of Persia and the Hedjaz are regarded as principal fostering causes of the disease, when it has passed the Indian frontier. Measures are, therefore, recommended for the sanitary control of these pilgrimages, and particularly with reference to the pilgrimage to Mecca. At Mecca measures such as those now finally recommended by the Conference were placed in force during the pilgrimage of last year (which occurred shortly after the Conference had assembled), and there is good reason to believe, with marked benefit to the health of the mass of devotees. The whole of the recommendations in regard to the hygiene of the sacred cities of the Hedjaz, and the regulation of the principal Mohammedan pilgrimage, are in the highest degree satisfactory, and full of promise if they should be carried into effect.

With regard to the protection of Europe from future invasions of cholera, the Conference thinks that this is not altogether impracticable. To this end, in addition to the internal measures of hygiene in India and the adjacent countries westward, it recommends a vast system of

quarantine of exclusion forming a permanent barrier to the migration of the malady within the European frontier. It assumes first that the evils arising from quarantine are of less moment and less distressing to commerce than the evils arising from an outbreak of cholera among a community. It assumes also that there is the highest probability that a quarantine based upon the knowledge of cholera which we now possess would prove successful. It admits that a quarantine of exclusion upon the coasts of the busy and crowded countries of Europe, where the populations are large and intercourse is great and incessant, would be a doubtful benefit; but it holds that where intercourse between community and community is slight, and towns and villages are widely separated, as on the confines of the East, quarantine would be effective and trustworthy. It holds, in short, that a quarantine of exclusion would be beneficial in proportion as it can be approximated to the great centre of infection, India, and applied to scattered populations. The Conference, therefore, recommends a system of quarantine extending from the mouth of the Red Sea to the Russo-Siberian frontier.

The routes between Persia and Russia both by land and the Caspian, also between Russia and Central Asia (the routes of cholera in 1831 and 1849), are already guarded by stations of quarantine observation, and the Conference suggests certain details of amendment for the better carrying out of the observation in respect to cholera.

A suggestion is made for the protection of Persia by the establishment of stations of observation on the routes from the Punjab to that country—an impracticable scheme, contrasting strangely with the comparative neglect of the route of infection by the Persian Gulf. For the latter, and principal route by which cholera reaches Persia, is left with seeming indifference, or with a better apprehension of the difficulties of erecting a barrier of quarantine among eastern nations than is suffered to appear elsewhere, to the protection of an ordinary quarantine.

With regard to the Turco-Persian frontier, it is recommended that every route shall be guarded, and that on the principal routes lazarets shall be provided. On some of the routes an inefficient sanitary observation has been kept up for some time by the Turkish Government, and the Conference looks upon this beginning as the nucleus of a more complete system. It admits, however, that its recommendations in respect to this frontier must for the present be looked upon as somewhat visionary—as a plan for study, not for immediate realization.

Having thus verbally provided for the safety of Europe from the migration of cholera by way of the Russian and Turkish frontiers, the Conference addresses itself to the task of barring out the disease along the route it followed last year for the first time—that is, by way of the Red Sea, Egypt, and the Mediterranean.

It recommends the establishment of a great international station of observation at the mouth of the Red Sea (on the Isle of Perim), with a lazaret on some convenient spot of the adjacent coasts yet to be determined. By this station it is believed that the direct importation of the disease from India would be prevented. But as the disease has never yet been known to have been imported into the Red Sea direct from India, the founding of so costly an establishment as that contemplated would surely be supererogatory. In 1865 the two ships which first

manifested the infection in the Red Sea, caught the disease at Makalla, on the South Coast of Arabia. Doubtless a station at Perim would prove effectual for the overhauling of ships so unfortunately circumstanced. But the chief line of transit of the disease in 1865, was not by ships which would come under the cognizance of observation, but along the coast, and probably mainly by coasting craft. Now, over this line of travel, a station at Perim would exercise no control whatever, and to establish it for the purpose of limiting incidental infection from the Arabian coast, would be a measure entirely disproportionate to the end aimed at.

In the event of cholera breaking out in the Hedjaz during the time of the pilgrimage, it is recommended that the intercourse between the Hedjaz and Egypt should be temporarily suspended, and that provision should be made for subjecting the returning pilgrims to quarantine at St. Wesch, a locality north of Jedda. These recommendations are plausible and probably practicable; even apart from considerations of the protection of Europe, the chance of saving Egypt from the disastrous consequences which have always followed invasion by the epidemic, would perhaps induce the government of that country to spare neither cost nor trouble to stay the onward progress of the disease. And the peculiar circumstances under which the pilgrimage is performed, and isolation of the Hedjaz, would admit of measures of restriction being adopted there, which would be impracticable elsewhere.

But supposing these measures should fail and the disease again reappear in Egypt, the Conference recommends that all intercourse between that country and the basin of the Mediterranean should be temporarily interrupted. This recommendation ignores the patent facts of the late outbreak. Here the rapidity and frequency of communication by the Mediterranean sea-route render it altogether improbable that a quarantine of exclusion would have the effect desired. Moreover, the recommendation is in direct contradiction with the facts of the epidemic extension of 1865. It is as certain as facts of this kind can be, that the diffusion of the choleraic infection in the Mediterranean in that year—at least at two points, Malta and Marseilles—had taken place before the disease was suspected to have existed in Egypt. And there is nothing in the suggestions of the Conference to shut out the likelihood of a repetition of such an occurrence. To stop the active intercourse with a country under such circumstances would be a vexatious folly. It could not offer any greater security than a well-regulated quarantine, and would give rise to multitudinous evils.

The recommendations of the Conference with regard to the formation of a barrier of quarantine between Europe and India do not beget confidence. They too manifestly extend much beyond the facts upon which they are presumed to be founded, and meet a theory rather than a practical need.

The subsidiary recommendations of the Conference deserve higher commendation. The suggestions as to the construction of lazarets, and the details of a quarantine against cholera; also in regard to the hygiene of cholera outbreaks in towns and cities and ships, and especially the application of disinfectants, are most instructive. We apprehend that the reports directed to these objects will prove the most valuable labours of the Conference.

IX.—*A Practical Treatise on the Physical Exploration of the Chest, and the Diagnosis of Diseases affecting the Respiratory Organs.* By AUSTIN FLINT, Professor of the Principles and Practice of Medicine in the Bellevue Hospital Medical College, and in the Long Island College Hospital, Fellow of the New York Academy of Medicine, &c. Second edition, revised. Philadelphia: Henry C. Lea, 1866. Large 8vo, pp. 595.

This is an admirable book. Excellent in detail and execution, nothing better could be desired by the practitioner. Dr. Flint enriches his subject with much solid and not a little original observation. For example, he has given great study to the character of the acoustic signs in diseases of the respiratory organs in relation to *pitch* as well as quality, and, he believes, with much profit. "By means of differences in pitch, conjoined with that of quality," he says, "the respiratory sign called bronchial or tubular breathing, may be readily distinguished from the cavernous respiration; a prolonged expiratory sound proceeding either from vesicular emphysema or an abnormal exaggeration of the vesicular murmur, that is, not denoting solidification of the lung, need never be confounded with the prolonged expiration which denotes a tuberculous or other solidifying deposit; exaggerated or puerile breathing is easily recognised as distinct from what has been called rude respiration; the vocal sign called bronchophony is distinguished from a simple increase of the resonance of the voice, and the pectoriloquy arising from solidifying lung is discriminated from the pectoriloquy which signifies a pulmonary cavity."

Dr. Flint also adopts several new terms. Thus, under the name *broncho-vesicular*, or *vesiculo-tubular respiration*, he describes certain modifications of the respiratory sounds representing all the degrees of solidifications of lung which fall short of an amount sufficient to yield purely bronchial or tubular breathing. "These modifications," he says, "have heretofore been loosely embraced under the names rude and rough respiration. The names broncho-vesicular and vesiculo-tubular express the distinctive characters of the sign, and are thus in themselves descriptive. By the different grades of modification, as regards the pitch and quality of the inspiratory and the expiratory sound, the amount, as well as the extent, of the solidification may be ascertained. This sign is of much value, especially in the diagnosis of tuberculous disease in its early stage." Dr. Flint, moreover, introduces the term *broncho-cavernous* as expressing the characters of a sign which represents solidification of lung and a cavity conjoined.

An original feature of the work is the introduction of several signs produced by the whispered voice. "These signs," he says, "as representing certain physical conditions, are generally available, and their characters relating to pitch and quality are highly significant. The names *exaggerated bronchial whisper*, *whispering bronchophony* or *bronchophonic whisper*, and *cavernous whisper*, although, perhaps, not intrin-

sically the best which might have been devised, have the advantage of corresponding with the names commonly applied to correlative signs produced by the loud voice."

Dr. Flint claims as a feature of his work "the recognition of the principle that the constancy of association of certain abnormal sounds with certain physical conditions constitutes the only reliable proof of the validity of the former as representing the latter. It is inconsistent with the principle to undertake to determine *à priori* signs to which certain physical conditions give rise, and still more, on the other hand, to infer the existence of certain physical conditions from certain abnormal sounds."

X.—*A Practical Treatise on Fractures and Dislocations.*

By FRANK HASTINGS HAMILTON, A.B., A.M., M.D., Professor of the Principles of Surgery, Military Surgery and Hygiene, and of Fractures and Dislocations in Bellevue Medical College; Surgeon to Bellevue Hospital and to the Charity Hospital, New York; Professor of Military Surgery, &c. in the Long Island College Hospital, &c. Third edition, revised and improved. Philadelphia: Henry C. Lea. 1866. Large 8vo, pp. 777.

This is the most complete treatise on the subject in the English language. The value in which the work is held in America is to be estimated by the rapidity with which the two previous editions have been sold and the necessity for a third has arisen. A better knowledge of its completeness and compendiousness, in this country, will lead to this volume obtaining a permanent place upon the bookshelves of practitioners. The experience of the recent war in America does not appear to have furnished Dr. Hamilton with so much additional material as might have been anticipated. He gives a summary of the statistics of the war in reference to gun-shot fractures. The question of trephining the spine, in case of fracture, will require reconsideration in a subsequent edition, from the additional light thrown upon it by Dr. Robert McDonnell. Not the least merit of Dr. Hamilton's work is the abundance of the illustrations. These are not quite equal in artistic execution to the merits of the book, although they serve the purpose for which they are intended.

XI.—*A Treatise on the Origin, Nature, Prevention, and Treatment of Asiatic Cholera.* By JOHN C. PETERS, M.D. New York: D. van Nostrand. 1866. Sm. 8vo, pp. 162.

Dr. Peters advocates the opinions now most commonly accepted respecting the transmissibility of cholera, and the mode thereof, and he clothes his principal propositions (without acknowledgment) mainly in

the words of Mr. Simon's important official memorandum on the subject. For the rest, he states that he makes "no claim for great originality" in any portion of his work, "except for the development of the Physiological Theory, and the advocacy of the internal disinfectant and corrective treatment." The theory referred to appears to be the dependence of the symptoms of cholera upon the exudation from the intestinal canal.

Dr. Peters's account of the origin and diffusion of the epidemic of 1865-66 is curiously incorrect.

XII.—*Club-Foot: its Causes, Pathology, and Treatment.*

Being the Essay to which the Jacksonian Prize for 1864, given by the Royal College of Surgeons, was awarded. By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopædic and Great Northern Hospitals. London: J. Churchill and Sons. 8vo, pp. 422.

The least merit of this book is that it is a successful prize essay. It has more substantial claims upon the consideration and confidence of the profession. It is the work of an accomplished surgeon, who has had large opportunities of acquiring an exceptional knowledge of the subject of which it treats, and we apprehend that it will long be the standard work on the subject in England. The book is thoroughly good in detail, illustration, and arrangement.

Mr. Adams first describes the various forms of club-foot, and the history of subcutaneous tenotomy in the treatment of the affection. He next discusses the questions arising out of the re-union of tendons after subcutaneous tenotomy, and the rate of extension after operation, in the treatment of club-foot. Then he proceeds to an examination of the relative merit of tenotomy and the stretching of muscles and other tissues by mechanical means in the treatment of club-foot. Next he proceeds to an account of non-congenital spasmodic and paralytic talipes, and of deformities with rigid muscles; also of the deformities with flaccid muscles, and with muscles in a healthy condition after recovery from paralysis, examining at the same time the pathology and treatment of these various conditions of distortion. Finally, he discusses in detail the different varieties of club-foot, and the special treatment of each variety.

Space prevents us following Mr. Adams over this large field. We may only dip into his work here and there. On the difficult question of the state of the tendon after the termination of the healing process when subcutaneous section has been performed, Mr. Adams remarks—

"The great difficulty in arriving at any satisfactory conclusion as to the ultimate disposal of the new connecting tissue, and the general results of tenotomy in the human subject, arises from the absence of facts which can only be acquired after death, and as there is nothing in the operation likely to produce death, the facts can only be obtained from some rare instances in

which death should happen to occur at different periods of the operation, from circumstances not connected with the operation, such as convulsive affections, acute pulmonary inflammation, or any of the acute diseases to which children fall victims, while the reparative process is proceeding uninterruptedly in a healthy manner. The *post-mortem* examination must therefore be extremely rare" (p. 24).

Mr. Adams's observations are, however, sufficiently numerous to show conclusively that an actual growth of tendon takes place, and that to this new growth is due the permanent elongation which follows subcutaneous section, and permits the rectification of distortion. He says :—

"The new tendon always remains a permanent tissue and as an integral part of the tendon, the divided extremities of which it has been formed to unite. In one specimen in which I divided the tendo Achillis three years previous to death, an inch and a quarter of new tendon was clearly traceable.

"The average length of new tendon formed in children operated upon for club-foot to re-unite the divided extremities of the tendo Achillis appears, from my observation, to be from half-an-inch to an inch, and in adults from one to two inches. I am of opinion the facts adduced are amply sufficient to disprove the *linear-cicatrix theory*, which assumes that a newly-formed tendinous structure has a disposition to undergo a process of gradual contraction, such as we see taking place in the cicatrices of the skin after burns, to which Mr. Tamplin has compared it, and that ultimately it becomes absorbed ; the muscular structure at the same time becoming elongated by the force of the contraction of the cicatrix, so as to allow of the re-approximation of the ends of the divided tendon, and the formation of a linear cicatrix.

"From my observation, it appears that, in the cure of deformities, muscles are elongated by the increased length of their tendons, obtained by means of subcutaneous division and the development of new tendon formed for the purpose of re-uniting the divided extremities of the old tendon" (p. 26).

On the general principles of treatment of club-foot, and particularly upon the combination of measures, Mr. Adams offers, among others, the following instructive remarks :—

"We dwell upon the necessity of this combination, because, in English orthopædic surgery at the present day, the operative and mechanical means are too generally adopted, to the exclusion or neglect of the physiological, the value of which increased experience daily brings before us ; and it is owing to this neglect that, although the external form of the foot in severe cases may be restored, a very limited amount of motion at the ankle-joint is gained, and a great tendency to relapse continues."

"If, then, without the aid of the physiological means, the operative and mechanical in combination are allowed to be inadequate to the cure of club-foot, so, on the other hand, he cannot too forcibly insist upon the fact that the operative treatment, or tenotomy, is the most unscientific that can be adopted unless the assistance of the mechanical and physiological is combined with it.

"In cases of club-foot, it is a great error to suppose that tenotomy constitutes the sole or even the chief remedial agent" (p. 32).

In an appendix Mr. Adams gives the histories of several instructive cases.

XIII.—*Asiatic Cholera: its Origin and Spread in Asia, Africa, and Europe; Introduction into America through Canada; Remote and Proximate Causes, Symptoms, and Pathology, and the various modes of Treatment Analysed.* By R. NELSON, M.D., Health Commissioner during the first two invasions, 1832, 1834; President of the Medical Board for the District of Montreal. New York: William A. Townsend. 1866. Sm. 8vo, pp. 206.

The interest of this book consists in the account which Dr. Nelson gives of the first invasion of Canada by cholera in 1832; and it is greatly to be regretted that he has not given the details (which he states that he possesses) of the dissemination of the epidemic from Canada into the United States. If the space given to the inutile discussion of the remote cause, pathology, and treatment of cholera had been occupied by the facts of its progress in 1832 which came under the personal or official knowledge of the author, his work would have been a welcome and permanently useful addition to the history of the disease.

XIV.—*The Antidotal Treatment of the Epidemic Cholera: with Directions, General and Individual, for the Prevention of the Disease.* By JOHN PARKIN, M.D., F.R.C.S., late Medical Inspector for Cholera in the West Indies. London: J. Churchill and Sons. 1866. Third Edition. 8vo, pp. 321.

Dr. Parkin believes that carbon, in its simple and compound forms, is an antidote for cholera. Of all its forms he holds carbonic acid to be the best. This asserted antidote he holds to be especially serviceable in the earlier stages of the disease, and particularly at the commencement of collapse. "In the state of confirmed collapse, when the circulation is entirely suspended, the same result can hardly be expected to follow; at least by the introduction of the remedy into the stomach" (p. 58). But even in collapse, Dr. Parkin states, as the result of his experience, that the treatment by carbonic acid "is infinitely more beneficial than any other method" (p. 81).

Of the mode of carrying out the treatment he says:—

"As it is absolutely necessary that the medicine should be taken in a proper manner—for, otherwise, the patient will only be swallowing a simple solution of tartrate or citrate of soda, instead of a certain portion of carbonic acid gas—it may not be superfluous to point out what I consider to be the best mode of preparing the effervescing draughts.

"Thirty grains of the powdered bicarbonate, or sesquicarbonate, of soda or potash should be put into a large tumbler with a wine-glassful of water, to which is to be added a dessert-spoonful of any simple syrup, mixing the

two ingredients together so as to form a homogeneous mass. Then take twenty grains of citric and tartaric acid, and dissolve them in half a wine-glassful of water, when the solution is to be poured on the contents of the tumbler, and the mixture drank off immediately, *before the effervescence has subsided*. If more convenient, or when to be obtained, lemon-juice may be substituted for the citric and tartaric acid, in the proportion of two table-spoonfuls of the lemon-juice to the same quantity of soda or potash. As the object in giving the syrup is to render the mixture more tenacious, and to prevent the gas escaping as rapidly as would otherwise be the case, it is not necessary, when the lemon-juice is used, to add any syrup" (pp. 69-70).

Dr. Parkin supports his opinion of the value of carbonic acid in the treatment of cholera by statistics showing happy results. But if, tempted by his experience, others should be disposed to follow his practice, they must not forget Dr. Parkin's limitation. "When the question has been put to me," he says, "Is carbon a remedy for the collapsed stage of cholera? my answer has been—No; it is not a remedy *for* collapse, but a remedy to *prevent* collapse" (p. 320).

Effervescing draughts such as those recommended by Dr. Parkin have been commonly used as adjuvants to other treatment; but Dr. Parkin makes these draughts the essential part of the medicinal treatment, or administers the carbonic acid or carbon in some other form.

XV.—*Cholera: its Seat, Nature, and Treatment*. By CHARLES SHRIMPTON, M.D., Chevalier de la Légion d'Honneur, Médaille du Choléra décernée par la Ville de Paris, 1832, &c. London: J. Churchill and Sons. 1866. 8vo, pp. 109.

Dr. Shrimpton's work will chiefly interest the English reader from the account which it contains of the discussions in France on the mode of appearance of cholera, whether spontaneously or by transmission, in Marseilles in 1865. On this question his work may be consulted with advantage. Dr. Shrimpton disbelieves the theory of importation, and carefully reports the evidence against this view. He discusses, also, the pathology and treatment of cholera.

XVI.—*The Tropical Resident at Home. Letters addressed to Europeans returning from India and the Colonies, on subjects connected with their Health and Welfare*. By EDWARD J. WARING, M.D., F.L.S., M.R.C.P., Surgeon in Her Majesty's Indian Army; formerly Acting Health Officer, Fort Morant, Jamaica. Sm. 8vo, pp. 242.

We shall not pretend to estimate the opinion which "old Indians" will entertain of this work, but it will certainly prove amusing as well

as interesting to residents at home who have not had any experience of the distant East. Dr. Waring brings the "old Indian" before the home public in a new light. He pictures him as an amiable innocent, who requires again to be put into leading-strings from the moment he touches the English shore. During the long absence from "home" and long life under the conditions compelled by a tropical climate, he is assumed to have lost in the main the habits, and forgotten the common state, of the home life. He is thus apt to form erroneous estimates of the land from which he has been alienated many years, and to become the prey of common-place schemers. We cannot dispute this picture: Dr. Waring, it must be assumed, writes from experience. This, however, broadly would seem to be his notion of his quondam and distant friends, and he kindly volunteers to advise them under the circumstances. He does this in a series of letters which all will read with interest, many with profit. He takes the "old Indian" by the hand, and first cautions him against the possibility of disappointment on his reaching home. Death and a re-distribution of friendships and interests will have operated many changes during his absence. Even his former intimate friends in the East may be swallowed up by closer ties at home. Then the contrast between the restraint of English life, and the freedom of the life he has just come from. All these things may tend to deaden the warmth of the reception he had hoped for on once more touching the dear old shores, and so lead to a painful disappointment. Who will not recognise the truth of the picture and sympathize in it? Dr. Waring, having cautioned the "old Indian" against such disappointment, next proceeds to lecture him upon the necessity of occupation, if he would be happy. Farming, horticulture, botany, and other natural sciences, outdoor sports and pursuits, literature, art, religion, philanthropy, and the volunteer force, are open to him, and form fitting fields for his exertions. Hear a fragment of what he says of literature:—

"It may, however, be very possible that with strong literary tastes you have no desire to appear in the character of an author. It makes little difference. You need never be in want of pleasurable and general occupation. To you, and such as you, London is, *par excellence*, the place of places. What noble libraries at command! Why, the reading room of the British Museum, open to all free of cost, is a place where a man like you might spend 'a year of Sundays,' without a single feeling of *ennui*. Fancy (and may the fancy soon be a reality) being comfortably seated in one of the most magnificent rooms in the world, with the power, by simply writing a name on a piece of paper, of being supplied with any book that your wildest imagination can fix upon; the costliest and the rarest books are there awaiting your call. But, besides this grand national collection, there are numerous other splendid libraries in every department of literature, either open gratis on presenting a letter of introduction, or available by the payment of a small annual subscription. Hard indeed to be pleased must that man be, who amongst such treasures should fail to extract pleasure and occupation enough for his mind, however active.

"But above and beyond these, are those delightful old second-hand book shops. . . ."

For the rest of the pleasant vision, we must, however, refer to the book itself.

Dr. Waring next chats with the "old Indian," in succession, on the

choice of a place of residence, the climatology of England, the search after lodgings, clothing on arriving, night attire, food, beverages, baths, physic, and income, and on all these subjects he offers abundant good and pleasant advice, not hesitating, when the occasion fits, to descend into minute details; witness, among other instances, the paragraph on night-caps.

XVII.—*A Winter in Paris: being a few Experiences and Observations of French Medical and Sanitary Matters gained during the Season of 1865-6.* By FREDERICK SIMMS, M.B. Lond. London: J. Churchill and Sons, Sm. 8vo, pp. 151.

This is a very pleasant gossip concerning things medical in Paris. Mr. Simms very agreeably describes the department of Public Assistance in its relations to the Paris hospitals, the hospitals of that city both special and general, the school of medicine and method of medical education, and he adds a notice of the sanitary arrangements of the quondam Lutetia.

XVIII.—*Transactions of the Pathological Society of London.* Vol. XVII. Comprising the Report of the Proceedings for the Session 1865-66. London: Printed for the Society. 8vo, pp. 482.

The yearly volume of the Pathological Society's Transactions increases in bulk and richness. The present volume is a fertile mine of pathological facts.

XIX.—*A Practical and Theoretical Treatise on the Diseases of the Skin.* By GEORGE NAYLER, F.R.C.S., Assistant-Surgeon to the Hospital for Diseases of the Skin, Bridge-street, Blackfriars. London: J. Churchill and Sons. 1866. 8vo. pp. 292.

Although several excellent and handy treatises on skin diseases have recently appeared, Mr. Naylor's work is most welcome. His descriptions of the different cutaneous maladies are very clear and good, and his account of treatment, verified by the large experience of the hospital to which he is attached, is full of instruction and suggestion. His treatise is certain to command favour with active practitioners. Additional items in its favour are the excellence of the illustrations and the goodness of the typography.

We shall dwell only on the last chapter of the work. This is devoted to some observations on diseases of the skin following vaccina-

tion. The agency of vaccination in the evolution of syphilis is especially considered, and Mr. Nayler takes exception to Mr. Simon's summing up of the question in his official papers on vaccination, published in 1857. "In these papers," Mr. Nayler writes, "there is no one point more strongly contested by Mr. Simon than the transmission of syphilis by vaccination. I must confess," he adds, "to my failure in the perception of any antecedent improbability in the question as to render its impossibility conclusive; or that vaccination should stand alone in its unqualified results. The objections taken by Mr. Simon may be reduced to these:—

1st. That the constitutional disturbance attending vaccination may of itself give rise to eczema, when a predisposition to this disease exists. Nothing can be more true, and how often do we find the same complaint accompanying dentition, or proceeding from an apparently trivial cause. Again, Mr. Simon takes exception to the rarity of the so-called cutaneous complaints consequent upon vaccination, as affecting the validity of their existence. This line of argument is untenable. The comparative rarity is admitted. It furnishes evidence the most direct in favour of the general care with which vaccination is conducted, but it proves nothing more; and 3rd, the assertion that syphilis has actually followed vaccination, is simply an assumption unsupported by fact.

Mr. Nayler continues:—

"The cases which I have selected occurred at the Skin Hospital, under the care of Mr. Startin, at a time when I was his clinical assistant.

"The first was that of J. W., aged twenty-two years, the youngest of a family of seven, who, herself excepted, have always possessed excellent health. Her father died of phthisis, mother still alive. At the age of three months she was vaccinated on the left arm, but the pustule never quite healed. Much inflammation followed, and the ulceration spread until it at length involved the whole arm, and showed all the characters of syphilitic lupus. The front aspect of the arm, from the shoulder to the elbow, presented a series of irregular cicatrices, with here and there a few tubercles ready at any time to ulcerate. On the face the disease commenced eight years ago, and is now represented by a patch on either cheek more than three inches in diameter. She has felt much pain at times, and before she came to the hospital in 1862, she had been subjected to various modes of treatment. Most benefit has been obtained from the exhibition of the bichloride of mercury and the iodide of potassium, and the external application of the arsenical caustic of the Hospital Pharmacopœia.

"The next is E. M., aged fifteen years, the youngest but one of five, and who was vaccinated in the usual manner on the left arm only a twelve-month ago. To the time of vaccination she was quite well, and a strong, healthy girl. The vaccine pustule soon became an unhealthy sore, occupied several weeks before it closed, and ended in a large irregular cicatrix. Before the latter had completely formed, psoriasis became developed on the lower extremities, even to the toes, and then proceeded to the upper and other parts of the body; the eruption resembled that of psoriasis guttata, but the scales were badly formed. She became an out-patient on the 21st of April, 1863, and was soon relieved by remedies similar to those used in the last case, and the employment of an ointment composed of creosote and mercury.

"The third case was that of M. M., nineteen years of age, who, on admission, presented the following symptoms; her entire scalp was a mass

of ulcers and cicatrices; the former varied in size from a sixpence to that of a penny piece, and exposed at different points the bone, much of which was in a necrosed state. Most of the ulcers were filled with a thin and very offensive discharge. Several soft tubercles might also be seen at the bridge of the nose and about the ears. She had never menstruated, and seemed much out of health; her hands were always damp, and feet cold. She was the third of six children, but unlike the others, had always been in indifferent health from the time of her vaccination, when three months old. The vaccine pustule soon degenerated into an ill-conditioned ulcer, which did not heal for twelve or more weeks. She had also abscesses about the arm. A cicatrix remains to this day, similar to that mentioned in the last case. At the age of fourteen years she was attacked with ser-piginous ulceration of the scalp, until it involved the whole of its surface. She was too ill, being unable to walk beyond a few yards, to continue her attendance at the Skin Hospital, and was therefore admitted into St. George's Hospital, under Mr. Pollock's care, June 15th, 1864. Her diet was carefully attended to, and she was ordered a mercurial vapour bath three times a week, besides taking sarsaparilla with the syrup of the iodide of iron daily. At first she made an objection to the bath, deeming herself too weak to bear it, but she soon became reconciled from the benefit she derived from its use. She gained weight, and in less than a month the head was covered with a granulating healthy surface; portions of dead bone were continuously removed by the application of a dilute sulphuric acid wash. At the end of two months more she was discharged, relieved of all severe symptoms and comparatively well."*

Surely Mr. Naylor does not pretend to rest so grave a conclusion as the transmission of syphilis by vaccination upon cases so imperfect as the foregoing? Was J. W. under experienced observation from three months old to the age of twenty-two? If not, what is the value of the early history of her case? Is there any necessary relation between the vaccination of E. M. and the psoriasis which followed, as to justify its being placed in a category of proofs of vaccino-syphilitic transmission? And what sufficiency of proof exists of the relationship of M. M.'s vaccination at three months and the ill-conditioned ulcer following, and the appearance of ser-piginous ulceration of the scalp and subsequent evils at fourteen years of age?

On referring to Mr. Simon's report, we find that Mr. Startin, in his reply to Mr. Simon's question, "Have you any reason to believe or suspect (a) lymph, from a true Jennerian vesicle, has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated person; (b) or that unintentional inoculation with some other disease, instead of the proposed vaccination, has occurred in the hands of a duly educated practitioner?" replies, "This is a difficult question to answer satisfactorily, as the reply must rest upon what is to be regarded as a 'true Jennerian vesicle,' as this vesicle in a subject suffering under constitutional or acquired syphilis, or from porrigo, or even scabies, might be still a 'true Jennerian vesicle,' though not a pure one; and these maladies I have many times seen transferred from such a vesicle. I have also seen the same maladies inoculated by public vaccinators from unintentional

* This patient, as well as the first, J. W., the author presented at a meeting of the Western Medical and Surgical Society.

vaccination, and such parties, I presume, are 'duly educated practitioners.'"

We may fairly assume, taking Mr. Nayler's cases as examples, that at least in reference to the most important question of transmission, that of syphilis, the hospital to which he is attached is not so rich in illustrations as Mr. Startin would have us believe. With every respect for Mr. Startin's experience, he does not appear to have supported his broad statement by instances. And giving all force to Mr. Nayler's observations upon the condition of the vaccine pustule and subsequent health of the vaccinated as elements of diagnosis, we should hesitate to attach so grave a conclusion as the transmission of syphilis by vaccination in any of the cases he relates.

The recent lengthened discussion on this subject in the Academy of Medicine, Paris, exhausted the facts which could be adduced in the affirmative. But the result was far from convincing, so numerous were the sources of fallacy. The whole question has been most ably discussed in Dr. Seaton's admirable article on *Vaccination* in Reynolds's *System of Medicine* (vol. i.), and his remarks upon the affirmative evidence are as follows:—

"Now the cases which have been brought forward, whatever ground they may give for caution (and in a matter of such extreme consequence there can never be too much caution), do not appear to me to afford this strict proof, or anything like strict proof; each one of them is wanting in some essential point, or is open to some source of fallacy. Either there was no evidence that the child said to have originated the syphilis was at any time syphilitic, or it was not known that the alleged syphilitic vaccination was not in fact a syphilitic inoculation *instead* of vaccination; or there was reason to believe that the syphilis which developed itself after the vaccination had an independent origin; or the facts were inquired into at too great a distance of time, and depended too much on the statements of ignorant persons to be wholly relied on. Thus in the occurrence at Rivalta, the circumstances were not inquired into till four months after their origin.

Dr. Seaton speaks with peculiar authority on this subject.

XX.—*Clinical Histories, with Comments.* By HENRY DAY, M.D., M.R.C.P., Physician to the Stafford County Infirmary. London: J. Churchill and Sons. 1866. 8vo, pp. 254.

Dr. Day's work will be read with much interest. The records, by a thoughtful observer, of the rarer forms of diseases coming under his observation, or the results of some special mode of treatment, will always command attention. The first of Dr. Day's histories is devoted to a subject of growing interest in this country—cerebro-spinal meningitis. Dr. Day records two sporadic cases of the disease. Cases have also been recorded by Dr. F. J. Brown, of Rochester, and Dr. Clapton, of St. Thomas's Hospital. These cases are of great interest in face of the recent epidemic extension of the disease in Northern Europe. Dr.

Day does not seem to be acquainted with Dr. J. Burdon Sanderson's valuable report on the outbreak of this disease last year on the Lower Vistula. The report, accessible many months ago, is now published in the Eighth Report of the Medical Officer of the Privy Council.

Other histories of special interest are those referring to the treatment of acute rheumatism by blisters; rheumatic fever without pain; epilepsy from peripheral irritation, also from hepatic congestion, hysterical facial paralysis, &c.

Dr. Day gives six cases of acute rheumatism treated by blisters, and he adds the following remarks:—

“I could, without difficulty, give pretty nearly a score of similar cases, in all of which the blistering proved of as marked and unmistakable service as in those I have just described; but as it would be tedious to do this, I shall satisfy myself, and I hope my readers, by saying that after the proof I have had, I cannot hesitate to express my belief that this method will be found a most valuable adjuvant in the treatment of a most troublesome and, in the generality of instances, a most painful disorder.

“In every case in which I have adopted it, the relief from rheumatic pain has been beyond all doubt immediate, and for the most part permanent, and in no case have I had the supervention of pericardial or endocardial inflammation after the application of the blisters—so that, as far as I can judge, it seems to endow the patient with a sort of immunity from these complications.

“It will be observed that my cases were none of them treated with blisters *alone*, and after what I have said upon ‘special remedies,’ it could not be expected that I should do so, for although thoroughly convinced in my own mind that Dr. Davies’ treatment relieves pain and prevents cardiac complications, I am equally convinced that every case must be, or at any rate should be, treated on its own merits. Each case may require some modification in its management, and *special remedies* must all be used to some extent in an empirical manner until the *special pathology* of rheumatism is better understood; and as Dr. Fuller has observed in his work on rheumatism, ‘what we want is far less the discovery of any new medicines, than the adaptation of our present remedies to the exigencies of each case.’

“In concluding these remarks I may add, that in the most extensive blisterings in this malady I have met with only one instance of strangury being produced, and this was very trifling in its nature and rapidly passed off; but I have in some few instances found the blistered surfaces difficult to heal, and indeed this circumstance has not unfrequently been the occasion of the patient being longer under treatment than would have been otherwise necessary, the rheumatism having quite gone, but the blistered surfaces remaining unhealed. In a Metropolitan Hospital, from want of room to meet the many urgent cases which present themselves, of course such patients would be discharged, or continued as out-patients; but in a Provincial Hospital, the smaller number of patients allows of these cases being detained until their entire condition is such as to admit of their at once resuming their usual occupations.”

The history of hysterical facial paralysis refers to the case of a man aged thirty-one years. A first attack had occurred immediately after hysterical laughing and crying, preceded by intemperance, and vanished suddenly, after some preliminary treatment, during a shower-bath. A second attack followed intemperance and sexual excess, but

disappeared upon the third or fourth day, after a turpentine enema and several doses of bromide of potassium, as administered also in the first case. Other histories of morbid states are equally interesting.

XXI.—*Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital. With an Appendix on the recent Epidemic of Cholera.* Vol. III., 1866. London: J. Churchill and Sons. 8vo, pp. 499.

The third volume of the *London Hospital Reports* is not less abundant in interest than its predecessors. Among the more notable papers is an elaborate one by Dr. Letheby, "On Spectrum Analysis, in relation to Chemistry, Pathology, and Medical Jurisprudence." The spectra of the blood, he states, with reference to forensic medicine, are so remarkable, that they become the means of discovering the presence of blood in very minute quantity. After describing the mode of procedure for the spectrum analysis of blood-spots in all cases of medico-legal inquiry, he remarks:—

"It thus appears that a very minute particle of blood may be made to furnish its characteristic spectra; and when, in a medico-legal inquiry, these spectra are compared with the known spectra of blood, treated in exactly the same way as the suspected matter, the results are very conclusive. In fact there are no real fallacies to the tests; for although many red solutions may produce stains upon clothing like blood-stains, and may give spectra which at first sight appear like one or other of the blood spectra, yet there are none which show all the characteristic appearances of blood under the action of different reagents. Few, indeed, if any, will stand the test of ammonia, which only brightens the absorption bands of blood, while it alters the appearance of other colours; and if there be any doubt in the matter, a little sulphite of potash will remove it, for this bleaches every colour which is likely to be confounded with blood. Among the reds which cut off the blue end of the spectrum, and exhibit black bands in the green, that are more or less like those of blood, are cochineal, lac-dye, alkanet, madder-red, and munjeet, dissolved in each case in a solution of alum; but on comparing the spectra side by side with those of blood, it will be at once seen that the bands are not the same, either in their position or character. In the case of cochineal in alum, for example, which is so very like blood that it might almost be mistaken for it, the two bands are nearly of the same width, whereas, in blood, the lower band is always the widest; and the reverse is the case with alkanet in alum. Besides which, none of these colours will stand the action of ammonia. Even the gravy of roasted meat, if it be not from underdone meat, which is more or less modified cruorin, does not give the same spectra as blood; for although it sometimes shows a dark and sharply defined absorption-band, a little below the line (D), like that of reduced cruorin, yet ammonia weakens it, and citric acid, with protosulphate of iron, produces no change in it, as it does with hæmatin. In fact, if the gravy is very dark-coloured, and has been strongly heated, it gives a spectrum like No. 1, Fig. 7, without any absorption-bands. There is, therefore, no colour, as yet examined, which can, with proper care, be confounded with blood. A few precautions, how-

ever, are always necessary to guard against possible sources of error, and to obtain the most satisfactory results" (p. 40).

"As examples of the delicacy of the test, and also of the time which may elapse after the blood has been drawn, before it loses its properties, the following may be quoted:—In the year 1849 I had occasion to make a medico-legal investigation of some blood-stains upon linen, and the specimens, which have been kept from that time to the present, have been recently examined both by Mr. Sorby and myself. The stains were of a brown colour, and were quite insoluble in water—showing that the cruorin of the blood had been completely changed into hæmatin; but on treating a piece of the stained linen not larger than a quarter of an inch in diameter, with a weak solution of citric acid, the colour was completely dissolved, and there was obtained a pale yellow solution, which, in its acid condition, hardly showed a trace of the characteristic blood-spectrum of oxidized hæmatin. When, however, it was made alkaline with ammonia, it exhibited the two faint bands in the green, which are characteristic of alkaline hæmatin; and on adding a minute fragment of protosulphate of iron, the spectrum of deoxidized hæmatin, with its double band in the green, was well seen. A like result was obtained with another medico-legal specimen of blood, dated 1851, and with some more recent specimens of blood which I have had to examine, as in the case of the Ilford murder, in September, 1865, and of Mr. Briggs in July following, and the Plaistow murder in November of the same year—in all of which cases the spectra are still very characteristic, although the blood, in every instance, is changed into the insoluble form of hæmatin. It thus appears that the characteristic properties of blood are not lost after a lapse of seventeen years, but that the spectra are still as distinct, and as well marked, as with blood of only a few months old" (p. 42).

Mr. Hutchinson contributes a highly suggestive article on "Herpes Zoster,"—a clinical lecture. He propounds the following "riddle:—" *"Is herpes zoster an exanthem or neurosis?"*

"If an exanthem," he says, "why is it not symmetrical, not attended by constitutional disturbance, and liable to spread by contagion? If a neurosis, why should it not relapse, why should it have stages, and how can it protect the individual against a second attack? There is no other neurosis which can be mentioned (neuralgia, for instance), which is not very liable to return again after cure. My own suspicion is that it belongs to neither of these classes, but that it constitutes a new group by itself; and further, I feel convinced that whoever may succeed in unravelling the mystery which at present surrounds it, must at the same time make a discovery in physiology" (p. 70).

Other important papers by Mr. Hutchinson refer to the results which follow injuries of the nerve-trunks, and injuries to the spinal column and its contents.

Dr. Little communicates an instructive series of notes on the unsuccessful and successful cases of saline alcoholic injections into the veins for the relief of collapse of malignant cholera, treated during the epidemic of 1848-49.

Two cases of poisoning by the external use of belladonna are recorded by Dr. Gosset Brown and Dr. Fraser. In both cases there was considerable mental disturbance, and neither case ended fatally. It was not previously known that the ordinary external use of belladonna might seriously affect the system, and the cases furnish an instructive caution.

Mr. R. Brudenell Carter relates two cases of acute suppuration in the knee-joint, in which recovery with free motion ensued. This singularly favourable result was to be attributed to the use of an ingenious mechanical contrivance for fixing the joint, which is thus described:—

“A splint, as light and thin as was consistent with the possession of the necessary strength, was cut from a flat piece of deal. This splint was long enough to reach from the tuber ischii to the os calcis. At the upper end it was about three inches in width, and it gradually tapered to an inch and a half at the lower end; so that, when in position, it was everywhere overlapped by the limb. It was padded by two or three strips of blanketing, and by a little cushion to fill the ham; and it was secured upon the centre-piece of a many-tailed bandage. This centre-piece was somewhat longer than the splint, so as to turn round the heel, and reach along the sole of the foot to the roots of the toes. The tails were rolled up and tacked to two pieces of tape, and the whole apparatus so fastened together, that it could be put into its place by once elevating the limb. The heel and malleoli were then protected by strips of soft leather, spread with lead plaster; the splint was placed in position, and the leg gently lowered down to rest upon it. The pad under the ham was accurately adjusted, a little cotton-wool placed to fill up any hollows, and then the tails of the bandage were laid down firmly and closely, from the toes upwards, and thoroughly secured by starch. Opposite the knee-joint, two tails on each side were left unstarched; but the starch was again applied above. The unstarched tails were pinned, so that it could be opened to renew some charpie placed over the wound to absorb the discharge. As soon as the starch had hardened, the limb was slung by tapes from a common cradle, so as to move freely from the acetabulum, and to allow the patient to lie in almost any position” (p. 176).

A paper by Dr. J. Langdon H. Down, on marriages of consanguinity in relation to degeneration of race, leads to conclusions modifying considerably those commonly accepted.

“My own researches,” writes Dr. Down, “conclusively show that in England, at least, every fourteenth idiot only is the child of cousins. But can it be as certainly shown that the relationship *per se* is the cause of the idiocy? I think not, and the analysis I have made clearly shows, that in the vast majority of such, so great in fact that it may almost be said to be universal, other causes were operating which were merely intensified by the relationship. Had the same care been exercised in the selection of relations as is displayed by the breeder of race-horses, vastly different results might have ensued; or were the practice of the coloured races of North America in force, of destroying all the weak, rachitic, and diseased children, the intermarriage of cousins would not have displayed the facts which I have furnished. Consanguinity has doubtless the power of aggravating any morbid tendency, as I believe it has of perfecting any good quality. Any statistics on the results of the marriage of relations are of doubtful value, unless they give the life-history of the progenitors. What a different aspect the whole matter assumes when this plan is adopted, will be apparent to the readers of this paper. Whenever a similar investigation is made, I believe it will be found, as in the subjects of my own inquiry, that consanguinity is only *one* of the factors, and not the most important one, in the production of deterioration.”

Dr. Down's conclusions are based upon the careful examination of the histories of 852 idiots.

Dr. J. Hughlings Jackson contributes suggestive papers on certain cases of cerebral hæmorrhage and the functions of the optic thalamus.

The appendix to the volume is devoted to the cholera epidemic of 1866, as seen at the London Hospital.

XXII.—*A Treatise on Emotional Disorders of the Sympathetic System of Nerves.* By WILLIAM MURRAY, M.D., M.R.C.P. Lond., Physician to the Dispensary, and the Hospital for Sick Children, and Lecturer on Physiology in the College of Medicine, Newcastle-on-Tyne. London: J. Churchill and Sons, 1866. 8vo, pp. 118.

In this little book Dr. Murray seeks to establish the relation which exists between the emotions and the viscera through the sympathetic tissues. He maintains two propositions (1) That the emotions injure the body most commonly by their effects upon the viscera, through the sympathetic system of nerves. (2) That there are disordered states of the viscera which powerfully induce those emotions which are injurious to the body; these effects are produced through a simultaneous disorder of the sympathetic system.

The argument is well sustained; and as an illustration of Dr. Murray's style, we quote the following paragraph from his chapter of general remarks on the *modus operandi* of dyspepsia producing emotional disturbance:—

“It would seem that the digestive tract may be seriously irritated as regards its sympathetic nerves, when no great amount of irritation in the ordinary sense of the word, exists. Such a paræsthetic, or hyperæsthetic state of these nerves seems to excite morbid emotion rather than pain or other sensations indicative of ordinary irritation in the part, and this is done chiefly by destroying the quality of that *visceral sense*, which is, as we have said, the substratum of the emotional states. In irritation of these sympathetic nerves then, we see that one of their special functions is at fault; and that function alone may suffer, just as in some affections of an organ of special sense, such as the eye, we find alteration in the special function of its nerve to be the first and only indication of disease.

“We may convey to the reader's mind the best idea of this by referring to an hypothesis which has sometimes served us good purpose in studying these diseases; it refers especially to the manner in which emotions, rather than ordinary sensations, are excited by the sympathetic nerve.

“We will assume that the *Will* excites a force, which manifests itself in muscular movements, and this force, generated in the cerebro-spinal system, *travels along the cerebro-spinal nerves*, and manifests itself in muscular movements of a voluntary nature; again, irritation of the cerebro-spinal nerves is conveyed to the brain, where it is received as a *sensation*. We aver that emotional impulses, on the other hand, *travel along the sympathetic system* of nerves as well as the cerebro-spinal, and so doing manifest themselves in various changes in the functions of the abdominal viscera. Conversely, an irritated or disordered condition of these viscera, producing irritation of their nerves (the sympathetic nerves), does not lead to a sensation of ordinary pain, but to an alteration of the ‘*emotional sense*,’ inducing an *emotional state* different from that of health.”

Dr. Murray's work is thoughtful and unpretending, and is an acceptable acquisition in the present dearth of English medico-psychological literature.

XXIII. — *A Practical Treatise on Apoplexy (Cerebral Hæmorrhage); its Pathology, Diagnosis, Therapeutics, and Prophylaxis; with an Essay on (so-called) Nervous Apoplexy, on Congestion of the Brain and Serous Effusion.* By WILLIAM BOYD MUSHET, M.B. Lond.; M.R.C.P., University Medallist in Medicine. London: John Churchill and Sons. 1866. 8vo, pp. 194.

In this work Dr. Mushet states that he has "attempted to extricate apoplexy as a substantive disease from an assemblage of symptoms, *i.e.*, from the multiform phases of coma. I am strongly impressed," he says, "that the main obstacle to a proper and simple understanding of the affection has been its confusion with every malady attended by unconsciousness, irrespective of pathological conditions; coma (the order) and apoplexy (the genus) having been almost invariably regarded as metonyms, loosely expressing a deeper or more pronounced degree of cerebral torpitude than their absolute and less definite congeners—*caras*, *cataphora*, and *lethargas*."

Dr. Mushet carries out his attempt by a thoughtful criticism of his subject, illustrated by the results of his own observation. As an example of his manner, we may quote the following observations on the presumed influence of variations in the supply of blood to the brain, and disease of the cerebral vessels, as causes of apoplectic seizures:—

"When we reflect that those pursuing the most laborious employments and taking the most violent exercise, as artisans, athletes, runners, divers, children, and what is more weighty, that those suffering from obstructive, simple hypertrophous, and other affections of the heart are not, in early life, prone to true apoplectic seizures, we must admit that simple modification in the conditions of supply of blood to the brain does not *singly* exercise any material pressure or other influence in the causation of apoplexy.

"Having disposed of the circulation within the cranium, and endeavoured to argue that irregularity or deficient balance in the venous, arterial, or capillary apparatus is inoperative, *per se*, to induce cerebral hemorrhage, I shall allude to the important changes which are met with in the vessels, and immediately consider how far these appear to contribute to, or avail in, its production.

"It need scarcely be premised, that most systematic writers are agreed as to the weighty influence of disease of the cerebral vessels and tissue in the production of apoplexy; yet by some, this is not regarded as a necessary concomitant, and, in published works, examples are incidentally quoted or referred to, whose history *à priori* precludes the probability of cerebral or arterial degeneration. Nevertheless, I am persuaded that if these apparently exceptional instances were minutely detailed and analysed, they would confirm the doctrine, that in all cases of *primary* idiopathic san-

guineous cerebral effusion, disease of the coats of the vessels previously subsists.

“From these remarks, it must not be imputed that I conceive textural alterations in the walls of the arteries to be the sole and efficient ætiological agents in apoplexy, as their frequent presence in subjects of mature and advanced age, without the supervention of any cerebral symptoms, would disprove such view. On the other hand, apoplexy is almost restricted to the afternoon of life, a period attended by degeneration of the vascular tunics. These facts cannot be overlooked; but, for the sake of convenience, I shall defer the discussion of the relationship between disease of the arteries and apoplexy until I enter on the consideration of the changes observed in the cardiac walls and orifices. Albeit, lest hesitation arise in accepting the truth of the *coincidence* of morbid alterations in the cerebral vessels of persons cut off by an apoplectic seizure, it is expedient to affirm that this had been particularly noted by the majority of observers.”

As a further illustration we quote the following observations on the diagnosis of cerebral extravasation:—

“In our present state of knowledge it is impossible to localize cerebral extravasation, or always to affirm positively even that hæmorrhage has occurred. In fact, it may be absolutely asserted that sanguineous apoplexy does not furnish one diagnostic, or rather pathognomonic symptom; yet collectively a certain series of objective phenomena, in most instances, will render an opinion tolerably certain. Cases are, however, occasionally presented, which are but slightly pronounced, especially on invasion, and prove very embarrassing. It must in addition be remarked that *all comatose diseases in their last stages simulate true apoplexy, and cannot be discriminated in default of their previous history, which is unfortunately often deficient or absent in patients found insensible and brought to a hospital.* The age may be of some assistance, as cerebral hæmorrhage is not common until after the meridian of life. The condition of the heart, if known beforehand, may also aid us. After the access of the disease this is difficult to examine, and the value of physical signs is doubtful, as coma, from whatever cause, is usually accompanied by oppressed quasi-hypertrophous action of the organ, in consequence of altered innervation.

“*Cæteris paribus*, in a case of coma, the ascertainment of previous temperate habits, non-existence of *arcus*, and the exclusion of cardiac ailment augur favourably. Flushing or pallor of the countenance is an unsafe guide, without due correction, as the face is for the most part pale in the worst or ingravescens attacks, and it may be pallid, with lividity, throughout, if the hæmorrhage be complicated with uræmia.

“The most certain indications of intracranial hæmorrhage are *sudden* hemiplegia, with more or less immediate and profound loss of consciousness—with or without rigidity or convulsion—stertorous breathing, deviation of the mouth, flushed face, and a full slow pulse. Tonic or clonic contraction of the muscles of the limbs frequently testifies to the co-existence of ventricular or arachnoid sanguineous effusion, and general paralysis of the extremities is usually associated with diffuse hæmorrhage or the moribund state. Not any constant or special symptoms (as Gall and Serres maintained) attend apoplectic extravasation into the cerebellum, which may serve to distinguish it from effusion into other parts of the brain. (Brown-Séquard.) In a very circumscribed apoplexy, the slowness or almost negation of symptoms may render a decision difficult or impossible. If an attack, apparently apoplectic, *entirely* subside, *i.e.* without sequelæ, hæmorrhage, if pre-existent, must have been exceedingly limited, and in the im-

mense majority of cases the symptoms will depend, not on hæmorrhage, but on other disorder."

The essay on (so-called) nervous apoplexy, or congestion of the brain and serous effusion, was originally published in the *British and Foreign Medico-Chirurgical Review*, and has been noticed in a previous volume of the *Half-Yearly Abstract*.

We commend Dr. Mushet's well-conceived and executed work to our readers.

XXIV.—*Acholic Diseases; comprising Jaundice, Diarrhœa, Dysentery, and Cholera. With a preliminary dissertation on Bile, the Bilious Function, and the Action of Cholagogues.* By ALEXANDER CHARLES MACLEOD, L.K.Q.C.P.I., M.R.C.P., F.R.C.S., Surgeon-Major in Her Majesty's Madras Establishment. London: J. Churchill and Sons. 1866. Sm. Svo, pp. 230.

Dr. Macleod sets forth a particular theory of which he claims the parentage. He holds that the different forms of alvine flux named in his title-page—to wit, diarrhœa, dysentery, and cholera, frequently depend upon an insufficient action of the liver. The bile, he maintains, is not, when free in the bowels, an acrimonious and irritating fluid, as commonly supposed, but the reverse. But he believes that a suspension of the function of the liver, with consequent absence of bile, is an inevitable cause of irritation in the intestines, which nothing but a reproduction of that secretion can assuage or remedy. He also believes that this irritation shows itself in various forms, affecting, under different conditions, different parts of the intestinal canal. As a consequence of these views, he endeavours to show that these varying forms of irritation in themselves respectively constitute specific diseases, as dysentery, cholera, and diarrhœa.

As an intercurrent argument, he thinks it probable that the intestinal tube acts vicariously for the liver in certain conditions of suspension of the functions of that organ.

The following is Dr. Macleod's summary of the forms of the disorders referred to:—

"The addition of bile being necessary for a due performance of the assimilative process and the provision of healthy chyle, it follows that every diminution of the needful amount must be followed by a corresponding defect in the work of digestion, of which we are occasionally furnished with striking indications in the character of the *excreta*.

"Thus, when there is a serious deficiency in the supply of bile, we have constipation; when this has lasted some time, the bowels, irritated by continued contact with 'hyper-cholericized' blood, pour out mucus, converting the previous obstipation into a diarrhœa, an operation explained in the preceding chapter.

"In some conditions of the system, dependent perhaps on malarious influence, the function of the liver appears to be altogether suspended; a

severer form of enteric irritation ensues, succeeded by ulcerative inflammation, and constituting the commonest type of 'acute dysentery.'

"During these temporary suspensions of the liver's function, it may be assumed that the kidneys, whose office is of so versatile and accommodating a nature, act in a subsidiary way, removing from the blood (in a form proximate or remote, or in both at once), those elements that the liver has failed to convert into bile, and which therefore must tend to accumulate in the blood.

"But whilst the function of the liver is thus temporarily suspended, suppose, from any cause, that of the kidneys simultaneously arrested.

"We know of only one complete example of this terrible complication, and the result is—CHOLERA" (pp. 59, 60).

To those who delight in curious pathological and physiological theories, Dr. Macleod's book will be an acquisition.

XXV.—*On Surgical Diseases of Women.* By I. BAKER BROWN, F.R.C.S., Surgeon to, and founder of, the London Surgical Home. Third edition, revised and enlarged. London: Robert Hardwicke. 1866. 8vo, pp. 366.

This edition of Mr. Baker Brown's well-known work is enriched with many additional cases. Chapters also are added on retroversion, retroflexion, and antelexion of the uterus, vaginismus and abdominal sections. The last-named chapter is substituted for that on ovarian dropsy in former editions.

The illustrations of the work, drawn on wood by Mr. Isaac Baker Brown, junior, assistant-surgeon to the London Surgical Home, are of unusual merit.

Of the newer matter Mr. Brown's observations on treatment of displacements of the uterus by incision of the os and cervix, and his remarks on the relationship of vaginismus and fissure of the rectum will particularly command attention.

XXVI.—*Notes on Health in Calcutta and British Emigrant Ships, including Ventilation, Diet, and Disease.* By W. H. PEARSE, M.D. Edin., Government Emigration Service. London: J. Churchill and Sons. Fcap. 8vo, pp. 160.

This is a most interesting and instructive book, by a gentleman who has had a large experience of the subject of which he treats. The chapter on ventilation is amply illustrated by the tabulated records of its state in various ships, and its tendency is shown in the following paragraphs:—

"The simple facts which I have endeavoured to illustrate are, that the foul air of the 'tween deck being hotter than the upper air, it must ascend,

and that no system of ventilation can be aught but pernicious which does not provide for this escape as its chief object.

"Table F. shows how great is this difference, even to the amount of 11° . Chimneys suitable for the escape of hot and foul air must be provided, and of such a kind as are adaptable to the greatly varied directions of the winds in which a ship is placed. If the observations I have noted have been justly made, it follows that these chimneys must be at the *two extreme ends* of the 'tween decks, thus meeting the circumstances of the two opposite directions of the wind relatively to the ship.

"One can scarcely lay too much stress on the fundamental importance of means for escape of foul air rather than means for the supply of pure air; yet the latter is very commonly that which is uppermost in the minds of those who have to do with ship ventilation. When chimneys are provided which are so formed as to allow the escape, the ingress down the hatches and skylight tubes of cold, pure, heavy air is tremendous. In the tropics, with the thermometer at 80° or 84° , this ingress is not felt, but when in cooler weather, the great draught down some of the hatches, &c., is most manifest, and the people have to be carefully guarded from its immediate power or shock."

The chapter on Calcutta coolie emigrant ships is full of suggestive facts, deductions and reflections. Thus, Dr. Pearse writes:—

"The coolie is an ill, rice-fed, ague suffering animal. When launched into the sea climate, his system takes on a change of rate; some of his natural or accustomed (life-dependent) vital acts cease or alter, and thus, in one direction or another, he goes on to show his (so-called) diseases—diarrhœa, dysentery, cholera, cough, &c.—viz., those which must and do happen in or of him, and which in truth are the natural states or tendencies of his body."

Again, in the chapter on the Coolie Emigrant's diet, Dr. Pearse observes:—

"The diet of the native of India is one of the most interesting and suggestive subjects for study in the whole range of the natural history of the human animal. He does on his most simple foods, sustain the longest fatigues and labours. His 'wind' is splendid; the functions are regular; the animal life is almost perfect. His food appears to be almost the very best for the necessities of nature. It appears, however, as if some one small essential want only, was not sufficiently supplied, from which, amongst other material states, arises the tendency to death; the ulceration of mucous membranes, the rapid sinking of the vital powers, in short, the varied deviating phenomena which his life presents. That which is arresting to the mind in the contemplation of the native and his diet is, on the one hand, the great and almost perfection of life and function on such simple and sparse food, and on the other, the sudden and rapid ceasing of life. This tendency to rapid death, which, in its different symptoms we call cholera, diarrhœa, dysentery, fever, &c., it appears not unwise to view as the natural course and action of his life and whole history. The sudden and personally appalling phenomena of cholera, &c., are liable to fix the attention on its outer and most prominent symptoms, rather than on the natural course or order. On other parts of nature, to which the human thought has lain itself, or to which man's attention and ideas have lit up, his errors of method have been of the same general order and nature. The vast periods, the actual course of time and evolving existences, and the consecutive necessity and oneness of the order of (so-called) catastrophic

phenomena, with the ordinary overlooked quiet rate of time and events, are forgotten. I conceive that whether we contemplate a revolution in society, the separation of the avalanche, the existence of animals and life on this globe, or the existence of diseases—cholera, &c.—in man, that our first recognition of these facts is that of surprise, but that the reason in its calmness, and humility, and hope, assures itself that such events are secondary phenomena of the greater, and containing necessitous and actual rates of time and matter and ‘forms.’ Thus, whatever the more potential circumstances of the native’s disease may be, the thousands of years on which he has subsisted on a spare, and, perhaps, too same a diet, must form one essential—though, perhaps, of very minor import—fact in the natural history of his passage to death, in those rates which we call cholera, &c., &c. The tenacity of life in other cases, is equally striking; children and adults lingering month after month with diarrhœa, &c., &c. The qualities of the food of this ancient race, must, by such examples, be shown to be near to a just supply for human necessities.”

Still again in the same chapter:—

“As I have before compared it, the native dies just as a September leaf falls; fundamentally, it is neither this wind nor that which brought down the leaf, but its period had come, its life was passed, the elasticity, the vital power, was no more in it. So the native may die, whether we call his dying-picture cholera, fever, diarrhœa, dysentery, or syncope. Fundamentally and for a hope of prophylaxis and treatment, we must first view these as his at present (under all the circumstances) natural, and of necessity, tendencies and courses.

“In allowing the mind to dwell on large, or wide, or general views, there is no occasion to lose sight of any clear and contained facts. A state (say cholera) into which races may fall and tend, may or may not give rise to creations of forces, poisons, &c., capable of infecting other men. But whilst it is not only a legitimate inquiry for us, in our present ignorance, to seek any fact which may cause, *e.g.*, this disease, be it in water, air, clothes, electricity, &c., it is not less important to remember that cholera, in a large, historical point of view, shows itself, or rather that men show it, at periods whose rates and dates of returns are not yet clear to us, and that it may be, and is, in the very nature of individual life, and therefore of races, to show long periods of vigour and health, with associated and equally of necessity breaks in that course. The so-called disease is as true, and natural, and necessary a phenomenon as is the period of vigour. The actual progress in time, of inorganic or organic existences, or in human history, is not truly seen until we, with equal clearness, recognise the general course, the developmental directions, periods, and times, and the directions of ceasing. Of these latter (hypothetically) may be cholera, diarrhœa, dysentery, in the natives of India. It will be the glory of some one to see that want which the native’s system does not get. Already the agriculturist supplies to the soil these wants which his failing plants need. The potato in Ireland, the vine in Madeira, had a cholera of some kind, and so the September leaf.”

Dr. Pearse’s unpretending book contains the material for a much more pretentious work.

XXVII.—*The Inductorium or Induction Coil ; being a popular explanation of the Electrical Principles on which it is constructed, with the description of a series of beautiful and instructive Experiments illustrative of the phenomena of the Induced Current.* By HENRY M. NOAD, Ph.D., F.R.S., Lecturer on Chemistry at St. George's Hospital. 2nd edition. J. Churchill and Sons. 1866. Fcap. 8vo, pp. 109.

This is simply an elaborate catalogue of the instruments of a special character, made and kept in stock by an ingenious philosophical mechanist. As a popular treatise on the induction coil, it is most defective and unsatisfactory; and its imperfectness in relation to the induction coil instruments used for medical purposes would be altogether inexcusable if it were not for the special object of the work to deal with the instruments of one maker only.

XXVIII.—*On Malignant Cholera ; its Origin, Pathology, Treatment, and mode of Prevention, with the occupations of 5568 males over twenty years of age that died in London in 1849, 1853, 1854, and 1866 ; with an Appendix on Cattle Plague as compared with Cholera and other human maladies ; its history, pathology, treatment, and future prevention. A Letter on Homœopathy ; its false inferences and hollow assumptions. With an Essay on the present state of the Medical and Veterinary Professions in this Country.* By EDWARDS CRISP, M.D., M.R.C.S., L.A.C. London : Robert Hardwicke. 1866. 8vo, pp. 135.

The title of Dr. Crisp's book sufficiently explains its contents. The general conclusions at which he has arrived concerning cholera are thus summed up :—

“1. That cholera is a disease occasioned by a specific poison that affects first the nervous centres, paralyzes the heart and the most important organs of secretion, the liver and the kidneys ; disorganizes the intestinal secretory apparatus, disintegrates the blood, thereby allowing a large portion of its serous constituents to escape from the system.

“2. That the assertion that the anæmic condition of the lungs, and the consequent sinking in cholera, depends upon spasm of the pulmonary arteries, is not founded upon correct physiological inference nor upon pathological proof.

“3. That the belief that the vomiting and purging are efforts of nature to get rid of the morbid matter from the system has little or no real foundation, and that the general adoption of the so-called eliminative or aperient treatment in the early stage of cholera is likely to lead to injurious results.

"4. That, judging from the occupations of 5566 males dying in London in 1849, 1853, 1854, and 1866 over twenty years of age (some of them engaged in the most dirty and filthy employments), occupation alone exercises but little influence in the production of the malady.

"5. That there is every reason to believe that under certain circumstances cholera is a communicable disease, but to a much less extent than small-pox, scarlatina, and other zymotic affections.

"6. That bad water, improper food, a vitiated atmosphere, want of cleanliness, intemperate habits, fear, the incautious use of aperient medicines, and other depressing agents, all act as predisposing and exciting causes of the disease.

"7. That the general belief that cholera is a disease confined especially to the poor and destitute is entirely disproved by the statistics that I have adduced.

"8. That the great majority of cholera cases, when seen at the onset in the first stage of choleraic diarrhœa, are for the most part readily under the control of opium, combined with astringent medicines.

"9. That, judging from our present knowledge, the calomel treatment when commenced early in the second stage of cholera and properly carried out offers the best chance of cure, but that, like every other medicine, in many cases it appears to have no salutary effect.

"10. That, as shown by my experiments, cholera evacuations produce no injurious effect upon fishes, reptiles, birds and quadrupeds; and that, from the evidence I have adduced, the lower animals and the members of the vegetable kingdom during cholera epidemics in this country have been as healthy as usual."

The latter portion of Dr. Crisp's work consists mainly of a series of letters addressed to the Lords of the Privy Council on the cattle-plague. Dr. Crisp publishes them verbatim because he is "anxious that they should form a record of the outbreak of rinderpest in this country, and of the tardy and inefficient steps taken by those in authority to stamp out the pestilence at an early period."

XXIX.—*On Diseases of the Respiratory Passages and Lungs, Sporadic and Epidemic. Their Causes, Pathology, Symptoms, and Treatment.* By WALTER GOODYER BARKER, M.B. Lond., Senior Medical Officer to the Worthing Infirmary, &c. London: J. Churchill and Sons. Sm. 8vo, pp. 282.

The object of this work, Dr. Barker tells us, is to illustrate the causes of lung-diseases, both sporadic and epidemic, which in these climates are of all others the most frequent and fatal; and he fears that from the very simplicity of these causes, they will fail to convince. He seeks to show that the pulmonary disorders of which he treats, occur in a direct relation to the exposure of the part affected to the outer atmosphere, and to its fluctuations. Thus coryza, bronchitis, and pneumonia occur in a proportion having a marked relation to the exposure of the Schneiderian membrane, the bronchial mucous membrane, and the more remote tissue of the lungs to the outer atmosphere.

As a further proof of his propositions, he adduces pleurisy. "Of all the diseases of the respiratory apparatus," he writes, "this is the least frequent, and as a rule, may be altogether prevented by appropriate clothing; and the reason is equally obvious, namely, that the pleural sac is entirely unexposed to the immediate contact of the external atmosphere."

Widening his field of observation, Dr. Barker endeavours to explain the atmospheric conditions under which cholera, fever, and zymotic diseases generally, are developed; also to show that the great dynamic agent in the production of these diseases is an elevated temperature. Dr. Barker's book will be read with interest.

XXX.—*On Diseases of the Veins, Hæmorrhoidal Tumours, and other affections of the Rectum.* Entirely re-written. By HENRY LEE, F.R.C.S., Surgeon to St. George's Hospital, &c. Second edition. London: J. Churchill and Sons. 1866. Svo, pp. 190.

This book will be consulted with no little profit. It is full of suggestive thought and practical lessons. Mr. Lee first describes the different affections which have been described at one time or other under the name of phlebitis. These he states are three distinct varieties of thrombosis, two of which, the earliest cited in the accompanying paragraphs, have no connexion with disease of the venous coats. These varieties Mr. Lee thus describes:—

"1. A spontaneous coagulation of the blood within the vessel, unconnected with the entrance of any extraneous material. This may take place in several cachectic states of the body, wherein there is a diminished force and movement of the circulating fluid, conjoined with anæmia, and an absolute or relative increase in its fibrinous constituent. Such are the cases delineated by Dr. Richardson and Mr. Humphrey.

"2. A coagulation induced by the entrance of some abnormal material into the venous current, and its action upon the circulating fluid.

"3. Coagulation caused by irritation, injury, or any disease affecting the venous coats themselves. Here the blood coagulates by virtue of some morbid impression made upon the vessel, which may be accompanied by injury to, roughening, elevation, or ulceration of its living membrane" (p. 17).

Mr. Lee next proceeds to discuss these different varieties and their treatment in detail, and respecting the latter, and with particular reference to purulent infection, he directs particular attention to Dr. Polli's experiments with the alkaline sulphites. He adds, however, that "if the results obtained by Dr. Polli and others be verified by clinical experience, *then* the merit of the discovery [the apparent neutralization of morbid ferments in the blood of living animals without altering it in a manner incompatible with life] can scarcely be over-rated. This, however, remains to be seen, and learning by every day's experience, the author would desire above all things to avoid all eulogistic or exaggerated terms" (p. 73). In a foot-note, Mr. Lee expresses the opinion

that, although the alkaline sulphites may prove useful in cases of putrid infection of the blood from the injection or absorption of decomposing agents, they will not be of much use in the ordinary forms of pyæmia.

Mr. Lee describes a special operation for varicose veins and varicocele, and he then proceeds to discuss, in more or less detail, some diseases of the rectum, particularly hæmorrhoids, polypi, and morbid states of the sphincter. On the treatment of hæmorrhoidal tumours by nitric acid, Mr. Lee has the following observations :—

“The benefit derived from this plan of treatment must not be expected till the small ulcers made by the caustic begin to heal. The loose folds of mucous membrane are then drawn upon, and the whole of the mucous lining is rendered more tense. Each small cicatrix, moreover, serves as a permanent point of attachment for the relaxed membrane, and consequently the inner coat (which alone descends in such cases) is retained permanently in contact with the other coverings of the bowel.

“The degree of pain experienced in this operation depends in a great measure upon the way in which the nitric acid is applied. The sensibility of the thin skin around the anus is very great; and if the acid be allowed to come in contact with it, the degree of pain is very considerable. If care be taken, on the other hand, to confine the application of the acid to the comparatively insensible mucous membrane, extremely little pain is caused. Nitric acid should never, therefore, be employed in cases of external hæmorrhoids, both because of its painful action on the skin, and because it is not the remedy required, that being excision by the knife or scissors.

“In the application of nitric acid to hæmorrhoidal tumours, the degree of irritation experienced in some measure depends upon the extent of surface involved in the operation. When therefore a considerable amount of the mucous membrane descends with the tumours, it is desirable to select certain portions, to which the application of the acid should be confined. The action of the acid may be limited, either by applying a small quantity at a time, or by shielding the surrounding surface with a paste made of chalk and water.

“The best mode of effecting this object is, however, to apply the nitric acid through a small speculum with an aperture in its side. Through this aperture the mucous membrane or pile will protrude, and may be effectually destroyed. As soon as this is accomplished, the surface should be smeared with the paste made of chalk and water, and the speculum withdrawn. So little inconvenience does this give, that the patient is generally not aware that the acid has been applied.

“Every portion of mucous membrane to which the acid extends should be as completely deprived of vitality as possible, since the degree of pain experienced will necessarily depend upon the remaining sensibility of the parts.

“Unless these conditions are observed, the application of nitric acid, or of any other caustic, to the mucous membrane of the rectum, may prove as serious an operation as that for which it is intended as a substitute.

“The acid used in such cases should be the strongest that can be procured. That which is usually kept by chemists under the name of the strong nitric acid does not effectually destroy the surface to which it is applied; and when used, it therefore produces more pain than the strongest acid, and cannot moreover be so certainly relied upon to accomplish the intended purpose” (pp. 106—108).

The work terminates with an account of some restorative operations connected with the rectum.

XXXI.—*Guy's Hospital Reports*. Edited by C. HILTON FAGGE, M.D., and ARTHUR L. DURHAM. Third series, Vol. XII. London: J. Churchill and Sons. 1866. 8vo, pp. 684.

The present volume of *Guy's Hospital Reports* is unusually rich in matters of interest.

Mr. J. Cooper Forster discusses the cases of hydrophobia admitted into the hospital since the year 1831. In some preliminary observations, he refers to a growing belief that cases of this disease have been more frequent of late, and he thinks that there may be some foundation for this belief. On the frequency of dog-bites he has the following remarks:—

“Attempts have been made to show that bites from dogs are more frequent now than formerly; we can only give the numbers for two and a half years at *Guy's Hospital*; and this cannot be said to show much, except that dogs (and not dogs only) are accustomed to bite. In the year 1864, 51 cases of dog-bites were brought to the hospital; 6 were bad enough to be admitted. In the year 1865 there were bites from dogs 47; and from horses, men, women, donkeys, a cat, a rat, and a fox, 15; in all 62. In the first six months of the present year there have been 46 cases of bites from dogs and 9 from other sources, 13 of which occurred in March and 13 in June. That the numbers of bites from animals have increased, therefore, is apparent, but whether the creatures were rabid or not it is impossible to say. We do not appear to gather much from these reports.

“In no one case yet have we seen the original bite and the patient subsequently affected with hydrophobia. The fact of our witnessing a larger number of bites during the last year or two than formerly is probably owing, as is a general increase in accident cases, to the removal of *St. Thomas's Hospital*.”

Mr. Forster endeavours to throw some light upon the question of the length of time between the bite and the appearance of hydrophobia. He thinks that we may draw some such conclusions as the following:—

“1. That when a bite has occurred on the face a rapid appearance of the disease may be looked for; that a few weeks, probably four or five, will elapse before evidence of it is shown, and therefore that if those few weeks pass over safely an immunity from the malady may be expected.

“2. That when the bite has taken place on the hand a still longer time, from five weeks to a year at least, must be looked forward to with much anxiety. It may be that in the single case among the ten in which a year elapsed before hydrophobia occurred there was some special reason for the delay; to this we shall advert when speaking of the treatment.

“3, and lastly. That when the clothes have been bitten through before the skin is injured some years may pass ere the disease occurs.”

Dr. James A. Salter contributes an interesting article on the teeth as passive organs of speech.

Dr. C. Hilton Fagge and Dr. Thomas Stevenson give the results, in an elaborate paper, of an experimental examination of the application

of physiological tests for certain organic poisons, and especially digitaline. The following are the chief conclusions derived from the investigation:—

“We have found that digitaline, when injected beneath the skin of frogs in the proper quantity, produces three effects with almost absolute certainty. These effects are—(1) a peculiar form of irregularity in the heart's action; (2) stoppage of the ventricle in the white, contracted state; and (3) retention of the voluntary power for at least 15 to 20 minutes, and often for a much longer time, after the heart has ceased to beat. All these symptoms have unmistakably been produced by extracts of complex organic liquids to which digitaline had been added before extraction, as well as by extracts of fluids which had been vomited by dogs poisoned with this substance, or which were found in the stomachs of these animals after death.

“Again, it does not appear that there is much difficulty in distinguishing the effects of digitaline from those of other agents which are poisonous to frogs. No substance which is to be obtained in this country, with the exception of squill, the *Helleborus viridis*, and perhaps the *H. niger*, is known to exert the same action. Of these, squill is most unlikely to be used as an instrument of murder or suicide; and the *Helleborus viridis* might, perhaps, be distinguished from digitaline by its purgative effects on man, and possibly, also, by the greater rapidity of its action upon frogs. Of course, however, we should not be justified in assuming the presence of digitaline, rather than the helleborus, in an extract of unknown composition which we found to produce the characteristic action on the frog's heart, merely because these effects occurred after a longer interval than when a pure extract of the helleborus has been administered.

“With reference to the toxic action of extracts of vomited matters and gastric contents, it does not appear that their effects are ever very similar to those of digitaline. The fact that these extracts are poisonous to frogs, while they are not at present known to be injurious to the higher animals, might be urged as a reason for discarding frogs as the subject of physiological experiments in medico-legal inquiries. We have not, however, been led to this conclusion. We have already given the reasons which induced us originally to employ frogs for this purpose; and the experiments which we have made on dogs have convinced us that the symptoms of poisoning by digitaline in these animals are far less definite and far more difficult of accurate observation than those produced in frogs.

“Unless some points of distinction should hereafter be discovered, the similarity between the effects of extracts of vomited fluids containing no toxic agent and those of *Veratrum viride*, staphysagria, and other vegetable poisons, will be a bar to the detection of these poisons by their physiological action.

“At present, too, we regard it as very doubtful whether the action of aconitina on the frog is so characteristic as to be available for the discovery of this alkaloid.

“On the other hand, we have not in any instance observed tetanic spasms to be produced by any substance which was not already known to excite such spasms in frogs. In our experiments, as in those of other investigators, veratria and theine have caused tetanic spasms when given to these animals. But no one of the other substances which we have already enumerated, nor any one of the extracts of gastric contents or vomited matters, has been noticed to produce this effect.

“It is true that, as the frogs were at first tied down, we had not very good opportunities of observing such spasms; but there is no reason to suppose that we ever overlooked their occurrence, and we therefore think that

our experiments, so far as they go, give increased stability to the 'frog-test' for strychnia."

In certain observations on the pathology of some of the Diseases of the Nervous System, also in a paper on Functional Disease of the Nervous System, Dr. Wilks discourses very instructively. For example, on a question of present interest he writes:—

"With regard to the loss of speech in right hemiplegia, I need scarcely say that my observations accord entirely with those of Dr. Hughlings Jackson, although the true explanation of this remarkable circumstance has yet to be discovered. It cannot be believed that the organ of speech is originally situated on one side of the brain only, and thus the explanation must be sought in some secondary cause. My colleague, Dr. Moxon, has offered a very good theory, which probably has much truth in it; it is to the effect that of the two halves of which the body is made up one is more especially educated, and that the other follows the movement by consent. This phenomenon, which appears so remarkable, is probably merely one instance of a general law of our bodies which has hitherto been overlooked; so far from being exceptional, I believe it to be only one example amongst numerous others, which shows how partial is the education of our muscles. In the case of writing, the fact is so evident that we have never thought of its importance in reference to the physiology of the brain: as most persons write with their right hand, a hemiplegia on this side deprives them of the power to write, whereas a similar affection of the left side has no such effect. Now, when we consider that the mechanism by which writing is performed is entirely the product of education, and that the various movements are guided by nerves which are themselves under the influence of the brain, it becomes evident that one side of the brain only obtains the guiding power. Hence, were it possible for the halves of the brain to be changed over, so that the limb should retain its activity, the power to write would, I take it, still be wanting. It might, of course, be acquired, and thus our hemiplegic patients again learn to talk as they did in infancy. It is probable that for those operations in which we can use both sides of the body equally, both sides of the brain have been educated alike; but since in many acts one side rather than the other has been put into use, it has followed that one half of the brain has been specially educated. As regards the movements of the limbs, this is self-evident, but it now seems to be equally true of the power of speech, and it may be true, likewise, of many other operations, as, for example, musical performances. So, again, with reference to the eye; I have heard of a man who was in the habit of using a theodolite with the right eye, and who could not employ the left for this purpose, although the sight was equally good.

"There seems to be different degree of loss of speech. Thus, in some cases there is a mere inability to articulate, whilst the patient is able to write; in others, a loss of memory of words until they are suggested; and in yet others a total forgetfulness of names, the patient giving everything a wrong appellation. It would be important to know whether these different symptoms are associated with distinct lesions; at present it is thought that loss of speech is associated especially with some disease in that part of the brain known as the island of Reil; and yet, at the same time, it is said that the loss of speech is nearly always connected with a hemiplegia. It follows, therefore, that, unless disease in the spot just named is sufficient to produce a hemiplegia, there must be an affection of the centres, extending towards the external parts.

"In all the cases which I have myself seen, and, if I remember rightly, in

those which have been recorded by others, there has been disease in the central ganglia, and I take it that this is necessary for the production of a hemiplegia. But the question remains, is it necessary that disease should advance beyond these parts in order to cause the loss of speech? According to the theory of the education of the one side to the disparagement of the other, a simple loss of power is all that would be required to produce this symptom; and if, as a matter of fact, loss of speech accompanies all right hemiplegia, then, assuredly, a small spot of disease in the central ganglia is sufficient. It may, indeed, be thought that a further extension of the mischief outwards to the grey matter of the hemispheres (especially in the region of the island of Reil) is essential to the further loss of all memory of words; and, on that supposition, we should be on the watch for cases in which this part is injured independently of the centres, and in which loss of speech would, perhaps, exist without any corresponding hemiplegia. Cases of loss of speech certainly occur without a paralysis of the limb, but I am not aware that they have been shown to be due to disease in the island of Reil. I have on more than one occasion seen loss of speech with right hemiplegia where the lesion was confined to the centres, but in these cases I believe that the failure of speech proceeded no further than an inability to articulate. Aphonia may also occur from disease in the pons Varolii, and I have thought that, by noting accurately this symptom in cases of disease lower down in the motor tract, we might obtain a proof of the importance of the anterior lobe in the production of speech. For example, a lady had disease in the pons Varolii; her tongue and soft palate were paralysed, so that she could not utter a word, but she could write down accurately all her wants. On the other hand, I am now seeing a patient who has somewhat similar symptoms, denoting a disease of the pons, and who has loss of speech; but before this was complete, and when she spoke intelligibly enough to make herself understood by her husband, she called things by their wrong names. The cause was clearly not in the brain proper, for the temporary paralysis which she at first had was on the left side.

"If we believe that, for the production of a persistent hemiplegia, some part of the motor tract must be involved—if we find that in nearly all these cases of loss of speech with paralysis of the right side, one of the central ganglia is affected—and if we also adopt the theory that this peculiarity is due to the education of one side alone—these facts would imply simply that the muscles of the tongue and palate had never learned to act in a certain definite manner; but whether the memory of words, being independent of the mechanism by which they are formed, requires another locality for its action, is very doubtful. This anatomical and physiological question merges into the old metaphysical one as to how far the idea must correspond with the outward sign. I apprehend that, according to Dr. Jackson's supposition, a disease of the left corpus striatum would necessitate a loss of the power of articulation, and, if it also involved the neighbouring cineritious structure, would likewise destroy the faculty of speech; while if the latter were the only function affected it would be surmised that that surface about the island of Reil was alone affected."

Again, of another question, or rather series of questions, now occupying much of the attention of English physicians, Dr. Wilks has the following remarks, which we cannot forego:—

"*Diseases of the Cerebellum.*—With reference to this part of the brain morbid anatomy has unfortunately as yet taught us but little; the diseases of this organ have not given rise to any symptoms so characteristic as to indicate their seat.

"Physiologists infer from experiments on the lower animals that it is the function of the cerebellum to harmonize and co-ordinate the various movements ; so that, when an animal is deprived of this organ, it staggers like a man when intoxicated. They believe that without a cerebellum we should still have the power of movement, but that we could not walk steadily, move our hands with regularity, nor eat with propriety. Seeing that all physiologists make positive statements with reference to this matter, it is certainly remarkable that this view is not supported, so far as I am aware, by a single clinical fact. We are, no doubt, acquainted with diseases attended with a tottering in the gait and a want of control over the movements of the body. Such is the disease known as Duchenne's paralysis, which (according to this author's definition) consists in a 'progressive abolition of co-ordination of movement, and an apparent paralysis contrasting with the integrity of the muscular force.' With this titubation (to adopt the new expression) we should suppose a disease of the cerebellum ; but this is not the case. It is the spinal cord which is said to be the part affected in this malady.

"It may be that in cases of disease of the cerebellum the patient is too ill to attempt to walk, so that his want of co-ordination escapes observation. But in none of the cases in my books have I observed anything more than a desire to lie quiet in bed, and an anxiety to be let alone, and these are common symptoms in other cerebral diseases. A boy was lately under my care in the hospital with an abscess in the cerebellum connected with disease of the temporal bone ; this boy was so weak that he could scarcely stand. The same thing was observed in a girl affected with a similar disease. Hence, if it were simply said that a removal of this organ diminishes the muscular power, no facts gained in the wards of the hospital could be mentioned in contradiction. Dr. Gull had for many months under his care a child with a tumour in the cerebellum ; this child was blind, but quite sensible. He was too weak to stand, and answered questions very slowly, the words being drawled out in such a deliberate manner that it seemed as if the end of the sentence would never be reached. In this case there was also large ventricular effusion.

"My belief is that in these cases there has been either much loss of power or at least an indisposition to move ; and I have never witnessed a case in which the power was present, proper control being alone wanting. In like manner, my cases in no way corroborate another opinion, that the cerebellum is the organ of sensibility. Disease of this part does not in any way affect the intellectual powers. Andral remarked this many years ago with reference to tumours."

Mr. Edward Cock describes a new method for establishing a communication between the bladder and the exterior of the body when the urethra has become impermeable. It is the last resource in certain cases—cases where the bladder must be got at somehow to save the life of the patient, and where yet it cannot be reached, either by ordinary catheterism through the urethra, or by tapping above the pubes or through the rectum. Mr. Cock calls the operation he suggests "tapping the urethra at the apex of the prostate, unassisted by a guide-staff." The mode of performance is thus described :—

"The only instruments required are, a *broad* double-edged knife, with a very sharp point ; a large silver probe-pointed director, with a handle ; and a canula, or a female catheter modified so that it can be retained in the bladder.

"The patient is to be placed in the usual position for lithotomy ; and it is of the utmost importance that the body and pelvis should be straight, so that the median line may be accurately preserved. The left forefinger of

the operator is then introduced into the rectum, the bearings of the prostate are carefully examined and ascertained, and the tip of the finger is lodged at the apex of the gland. The knife is then plunged steadily but boldly into the median line of the perineum, and carried on in a direction towards the tip of the left forefinger, which lies in the rectum. At the same time, by an upward and downward movement, the vertical incision may be carried in the median line to any extent that is considered desirable. The lower extremity of the wound should come to within about half an inch of the anus.

“The knife should never be withdrawn in its progress towards the apex of the prostate; but its onward course must be steadily maintained, until its point can be felt in close proximity to the tip of the left forefinger. When the operator has fully assured himself as to the relative positions of his finger, the apex of the prostate, and the point of his knife, the latter is to be advanced with a motion somewhat obliquely either to the right or the left, and it can hardly fail to pierce the urethra. If, in this step of the operation, the anterior extremity of the prostate should be somewhat incised, it is a matter of no consequence.

“In this operation it is of the utmost importance that the knife be not removed from the wound, and that no deviation be made from its original direction, until the object is accomplished. If the knife be prematurely removed, it will probably when re-inserted make a fresh incision, and complicate the desired result. It will be seen that the wound, when completed, represents a triangle; the base being the external vertical incision through the perineum, while the apex, and consequently the point of the knife, impinges on the apex of the prostate. This shape of the wound facilitates the next step of the operation.

“The knife is now withdrawn, but the left forefinger is still retained in the rectum. The probe-pointed director is carried through the wound, and, guided by the left forefinger, enters the urethra and is passed into the bladder. The finger is now withdrawn from the rectum; the left hand grasps the director, and along the groove of this instrument the canula is slid until it enters the bladder.

“The operation is now complete, and it only remains to secure the canula in its place with four pieces of tape, which are fastened to a girth round the loins. There will probably be no escape of urine until the stilette is removed from the catheter.”

The necessity of micturating through the perineum, Mr. Cock observes, may seem a considerable hardship, but he points out that the inconvenience is not very great, and that it is to be remembered that the man's micturition is merely assimilated to that of the other sex.

Dr. Pavy reports a case of *vitiligoidea plana et tuberosa*; and Mr. Joseph Towne discusses the physiology of binocular vision.

Dr. Habershon offers some clinical remarks on the treatment of diseases of the heart. The principles of treatment he lays down and discusses in succession, are—1st. To lessen the work of the heart; 2nd. To insure regularity of action; 3rd. To lessen the distension of its cavities; 4th. To prevent syncope; 5th. To strengthen the muscular fibre of the heart; 6th. To hinder the fibrillation of the blood in the heart and great vessels; and 7th. To prevent secondary complications, and to relieve them when produced.

Dr. Habershon also reports a case of abdominal tumour.

Dr. Alfred S. Taylor contributes a detailed report of twelve cases of

poisoning apparently from the use of copper for culinary purpose; Dr. Gull an account of a case of intermittent hæmatinuria; and Mr. Birkett, observations and cases illustrative of the practical surgery of new growths and tumours.

In a paper on the Urine in Acute Rheumatism, Dr. Thomas Stevenson arrives at the following conclusions:—

“1. In acute rheumatism, when the excretion of solid materials in the urine is large, the patient makes, other things being equal, a rapid recovery; on the other hand, in lingering cases the excretion of solids is usually small.

“2. As in this disease the urine is invariably scanty in bulk, but (generally from this cause only) of high density, a useful guide to the progress of the case may probably be found by diluting the urine to the normal bulk, and then ascertaining its specific gravity. According as it is now of high or of low density will the progress of the disease probably be favourable or unfavourable.

“3. Though the excretion of urea is usually greater during the height of the disease than during convalescence, this is not invariably the case; the reverse sometimes occurs. Though the excretion of urea is greater during the disease than during the early stage of recovery, the urea in the former stage seldom very much exceeds in amount the normal physiological excretion.

“4. The uric acid is always much increased whilst the disease continues.

“5. The phosphoric acid is generally in greater amount during the progress of the disease than during recovery, but the quantity of this substance rarely much exceeds the quantity secreted in health.

“6. The excretion of sulphuric acid is generally increased, and often largely. In one instance more was excreted during recovery than during the acute stage of the disease. The amount of this substance excreted is very variable.”

A case of excision of the spleen for an enlargement of the organ attended with leucocythæmia is put on record by Mr. Bryant; and a remarkable case of spontaneous cure of aneurism of the aorta by Dr. Moxon.

Dr. Owen Rees illustrates the treatment of acute rheumatism by lemon-juice in a series of cases. He states that complications, especially of the heart-membranes, scarcely ever occur during the lemon-juice treatment, and that when present in the cases they have existed before the exhibition of the remedy.

Dr. Henry G. Sutton makes a further report of cases of acute rheumatism treated by mint-water, with especial reference to the natural history of the disease. The results are thus stated:—

“Of twenty-nine cases of rheumatic fever admitted into Guy’s Hospital during the last year, in sixteen there was evidence, on admission, of heart disease. Of these sixteen, eight appeared to have mitral disease on admission, and the same when discharged. Four of the sixteen appeared to have aortic valve disease (or there was a systolic murmur at the base, which was carried along the aorta). In the remaining four there was a murmur on admission, which disappeared as the patient became convalescent. In one case there was an anæmic murmur. In one fatal case there was early pericarditis. In one case there was no murmur on admission, but one was developed within two or three days, it appeared to be due to pericarditis,

and in the same case a mitral murmur was heard when the patient was convalescent. In ten cases there was no murmur heard, either at the time of admission, during treatment, or during convalescence.

"Twelve of these twenty-nine cases were treated with lemon-juice; seven with mint water; three with mint water and a little opium; three with the fixed alkalies; two with alkalies and lemon-juice; one with blisters. One patient was kept in blankets, no medicine being administered.

"Of the ten cases in which the heart was free from any bruit, five were treated by lemon-juice, two by alkalies, one by blisters, one by blankets, one by mint water.

"It may be noticed that in only one case was a bruit developed while under treatment, which may suggest the question as to whether it is common in hospital practice to find heart disease developed while under treatment.

"It seems to be far from common according to the experience of physicians who advocate different systems of cure. But it appears to us, that until it is decided what is the proportion of cases which escape heart affection with treatment by rest and light diet only, it will be very difficult to decide as to the amount of influence any system of cure has exercised in preventing such disease."

Other papers are as follows:—Notes on Cases connected with Obstetric Jurisprudence, by Dr. J. Braxton Hicks; Select Clinical Reports, by the late Dr. Barlow; Cases of Operations on the Larynx, by Mr. Arthur Durham; On Diseases of the Retina, with Remarks on its Structure and Normal Conditions, by Mr. C. Bader; and Clinical Remarks on Perforations and some other Morbid Conditions of the Membrana Tympani, by Mr. James Hinton.

XXXII.—*St. George's Hospital Reports*. Edited by JOHN W. OGLE, M.D., F.R.C.P., and TIMOTHY HOLMES, F.R.C.S. Vol. I. 1866. London: J. Churchill and Sons. 1866. 8vo, pp. 444.

The St. George's Hospital medical staff is following the excellent example set by the staffs of Guy's, St. Bartholomew's, and the London Hospitals. The first volume, issued under the joint editorship of Dr. J. W. Ogle and Mr. Timothy Holmes, abundantly shows that, as was to have been anticipated, another rich mine of clinical observation is opened out. The contents of the present volume are varied, and full of instruction. An exceedingly interesting account of St. George's Hospital and Medical School, by Dr. Page, very properly opens the volume. This is followed by the first of a series of contributions to the surgery of the head, the present paper being devoted to deviations of the base of the skull in chronic hydrocephalus, by Mr. Prescott Hewitt. Mr. T. Holmes next relates a case of meningocele in the occipital region, which was injected with iodine, without ill consequences, the patient dying of broncho-pneumonia. Dr. R. E. Thompson contributes a report on the typhus epidemic of 1864-5, as observed at St. George's Hospital; and Dr. Clifford Allbutt, physician to the Fever Hospital, Leeds, gives an account of the same epidemic as seen in that town.

The following observations from Dr. Thompson's report, as treating a great question of hygiene, are noteworthy. He writes:—

“The generation of the fever-poison was traced, with some plausibility, in most cases to overcrowding. Families were seized with the fever who, it was ascertained, slept, four, five, or seven in a room unfit for such a number. In isolated cases, where no previous case of fever was known to have existed, the same cause seemed to have given rise to the disease. A policeman was admitted who slept in the station with seven others, the beds being so close together that they could be easily touched by the arm. Other solitary cases were admitted from asylums and refuges where close packing existed.

“The disease was communicated to ten persons in the hospital: of these, five were patients; the others were three nurses, myself, and the night-superintendent—a most active and intelligent woman, whose loss was very much regretted. Out of these ten cases, two nurses, one patient, and the night-superintendent died. It will be well to mention that there is no special fever-ward in this hospital; the cases were mixed indiscriminately in the wards. And as the question of separation or admission of fever-cases into the general wards of an hospital is one of much importance, a table has been drawn up, giving the cubical contents of each ward, the number of fever-cases admitted, and the number of persons to whom (and in what ward) the fever was communicated.

“The patient who died of fever caught in the hospital took it from a single patient in a surgical ward, and is therefore not included in the table.

“To meet the exigencies of such an epidemic, where numerous cases of one kind are brought in, a special ward is, no doubt, best adapted; but for ordinary occasions there is much in favour of admitting fever-cases into general wards; but this precaution should be observed, that they be put at the end of the ward, and that other patients be strictly prevented access to them. By multiplying the foci of fever in a special ward, the risk is very much enhanced for a patient in whose case an error of diagnosis has been made, and for the medical officers who attend the special ward; and unless each kind of fever has its own ward, what immunity can be expected for patients variously affected? One of the patients who caught the fever in the hospital was originally admitted with measles; this was followed by scarlatina, and this again by typhus. Another patient, in like manner, originally admitted with scarlatina, was attacked by typhus; and a man admitted with typhus, at the end of 1864, subsequently died from a second attack of the fever. There is not a little difficulty in admitting statistics in the matter: it is easier to obtain evidence of the number of cases in which fever has been communicated in the hospital, than to collect the numbers of cases in which errors of diagnosis have been made, and in which the patient has contracted the fever in special wards: and, in addition, to estimate the increased risk to medical officers and nurses which such special wards afford.”

The subsequent observations on the treatment of the disease, by Dr. Allbutt, will also be read with interest:—

“It is difficult for me to say, without apparent exaggeration, how important a part I believe opium to play in our remedial system. We date a marked decrease of our mortality from the day that the windows were all thrown open; on which day also an order was given that no patient was to pass two sleepless nights running, if opium could prevent it. One year ago I looked upon opium in fever with much suspicion, granting its occasional

value only, and greatly restricting its use. Gradually my fear of the restlessness overpowered my fear of the opium, and I have now ceased to regard the latter with any great apprehension. Continually I have witnessed the terrible havoc which a night's tossing makes in a little reserve of strength. Two such nights reduce it to a most precarious level, and few patients outlive three. On the other hand, though I and my assistants have for six months been giving morphia at all stages of the fever to combat sleeplessness, I have never yet seen mischief result. I have never seen the power of taking food suspended by it, or the oppression increased. On the contrary, I have continually seen with pleasure how, on the morning after opium has brought sleep, even during the first few days of the disease, the tongue has become moister, the headache less, and the countenance more open. The sleep of an opiate is better than no sleep.* Camphor is a great favourite of ours also; we find, on the whole, that no medicine equals it in the low delirium often connected with feeble heart. We combine it with opium in low delirium, accompanied by sleeplessness. Perhaps, however, the most striking in its immediate effects of all the medicines which we have used is that which we familiarly call Graves' medicine. The combination of antimony and opium in the wild delirium of fever is advised by Graves in a well-known passage of the *Clinical Medicine*; and a marvellous remedy it is. Half a grain of morphia combined with one-third or one-half of a grain of the tartar emetic, with repetitions of half the dose, if necessary, will bring quietness and sleep to a patient who an hour or two before was a raging maniac, leaping from bed, and destroying all he could lay hands upon. Such patients far too frequently die of exhaustion; and many did die until we made a regular practice of giving the morphia and antimony. These, then, were our measures:—

“1. An unusual supply of fresh air night and day throughout the hospital, all fear of draught being disregarded.

“2. Regular nursing and feeding, and the use, when necessary, of the best cognac brandy in addition.

“3. Prevention by morphia, if possible, of a second sleepless night, at whatever stage of the fever it may be threatened.

“4. The use of a combination of camphor and morphia in low delirium.

“5. The use of a combination of tartar emetic and morphia in wild delirium.

“The class of cases which we had to treat were true maculated typhus, often presenting a very dull dusky rash and great prostration. The large majority of them were sent in by the guardians of the poor, and were of the lowest order of the people.†”

“* The quantity of morphia to be given depends much, of course, on the patient. I seldom, however, venture upon more than a grain or a grain and a third, for fear of collapse, and this in divided doses. Of course opium is withheld when any tendency to coma is seen. Where, however, as is not infrequent, convulsions of a tetanic order appear, we use opium often in large or repeated doses with the happiest results. Such convulsions appeared and continued from time to time during the 9th, 10th, and 11th days of the fever in the case of our resident medical officer. They were controllable by opium to some extent; and by its unsparing use I believe his life was saved. Under treatment by blisters, purgatives, &c., such patients, as far as I have seen, invariably die. I have not found that a weak or fatty state of heart is a bar to the cautious use of opium, though in such cases I always combine it with camphor and other stimulants.

“† By this I do not mean to imply that our cases of fever occurred in

Mr. Lockhart Clarke discusses in an elaborate paper the diagnosis, pathology, and treatment of progressive locomotive ataxy. Of the pathological anatomy of the disease, he says:—

“In true locomotor ataxy the spinal cord is invariably altered in structure. Its membranes, however, are sometimes apparently unaffected, or are affected only in a slight degree; but generally they are much congested, and I have seen them thickened posteriorly by exudations, and adherent not only to each other, but to the posterior surface of the cord. Now the posterior columns, including the posterior nerve-roots, are the parts of the cord that are chiefly altered in structure. This alteration is peculiar, and consists of atrophy and disintegration of the nerve fibres, to a greater or less extent, with hypertrophy of the connective tissue, which gives to the columns a greyish and more transparent aspect, and in this tissue are embedded a multitude of corpora amylacea. Many of the blood-vessels that traverse the columns are loaded or surrounded, to a variable depth, by oil-globules of different sizes. For the production of ataxy it seems to be necessary that the changes extend along a certain length—from one to two inches—of the cord. The posterior nerve-roots, both within and without the cord, are frequently affected by the same kind of degeneration, which sometimes extends to the surface of even the lateral columns, and occasionally along the edges of the anterior. Not unfrequently the extremities of the posterior cornua, and even deeper parts of the grey substance, are more or less damaged by areas of disintegration. The morbid process appears to travel from the centre to the periphery—that is, from the spinal cord to the posterior roots. In the cerebral nerves, on the contrary, the morbid change seems to travel in the opposite direction—that is, from the periphery towards the centres. From the optic nerves it has been found to extend as far as the corpora geniculata, but seldom as far as the corpora quadrigemina. With the exception of the fifth, seventh, and eighth pair, all the cerebral nerves have occasionally been found more or less altered in structure.”

In a paper on Rheumatic Iritis, Mr. James Rouse deduces—1st. That in early and slight cases of the disease, alkalies combined with colchicum and aconite are remedies effectual to arrest it. 2ndly. That adhesions once having been formed between the pupillary margin and the capsule of the lens, the use of mercury in some form is essential. 3rdly. That atropine is always necessary during some stage of the attack. 4thly. That in cases of severest pain and slight tension, the operation of cylectomy, or paracentesis through the cornea, is advisable: and 5thly. That in cases of recurring and chronic iritis, iridectomy is absolutely necessary to prevent blindness.

the worst possible subjects. Coming from destitute and over-crowded dwellings, they often showed symptoms of alarming prostration; over which symptoms, however, opium and brandy clearly had some control. The cases which I saw in private practice were on the average of a more dangerous kind, and the mortality higher. In persons accustomed to live by use of the brain the weight of the disease often fell upon that organ, causing cerebral and cerebro-spinal disturbances of an unmanageable and incalculable character, which tended to death. Among those who lived by bodily labour, and had no brains to speak of, the disease fell chiefly upon the muscular system, causing failure rather at the heart, and general animal and organic prostration; symptoms more easy to combat, and more easily foreseen in their variations and issues.

An article on Cerebral Symptoms occurring in certain Affections of the Ear is given from the pen of the late Mr. Toynbee. Mr. Warrington Howard discusses some points in connexion with hernia; and Mr. T. Holmes treats of Amputation at the Hip-joint, and on the applicability of this operation in some of the worst cases of Morbus Coxarius.

Dr. J. W. Ogle contributes an important paper on Disease of the Brain, as a result of Diabetes Mellitus, illustrated by the Narrative of a Case, in which Paralysis, due to Softening of the Brain, came on in a Diabetic Patient, and proved fatal. Dr. Ogle first directs attention to the remarkable physiological experiments on the lower animals, by which, through injury inflicted on various portions of the nervous system, glycosuria has been induced, and which have prepared us to expect, and directed us under certain circumstances to the discovery of, an undue amount of sugar in the urine as a result of lesions, and of disturbances of the nerves, spinal cord, and brain in man.

The object of Dr. Ogle's present communication "is to direct attention to the existence of cases of diabetes in which this order is inverted—of cases, that is, in which lesions and disturbances of the nervous system appear to stand in the relation of *consequence* or *result* of the diabetic state."

This is altogether a new aspect of the subject, and does not seem to have been noticed by previous writers.

"Neither general and systematic, nor special writers on diabetes mellitus," says Dr. Ogle, "have found occasion to include actual lesion of structure of the brain or spinal cord as a *result* or *sequence* of this malady. The close and continued watching, however, of a typical case of the disease in question, for a period of several months, has compelled me to the conclusion that whatever may be the more usual relationship between diabetes and disease of the brain, when they are found to occur in the same patient, there are cases in which brain lesion may follow the train of diabetes, and grow out of it, being in no wise antecedent to or the cause of it."

Dr. Ogle relates the case alluded to in detail, and follows it by a series of most suggestive and instructive clinical observations.

Other papers in this volume are: On Jaundice and Biliousness, by Dr. H. Bence Jones; on Paralysis in Childbed, by Dr. F. F. Fussell; Remarks upon the Modus Operandi of Hypodermic Injections, by Mr. C. Hunter; on Congenital Dislocations of the Femur, and on Talipes Varus, by Mr. Brodhurst; on the Diurnal Variations of the Temperature of the Human Body in Health (a most instructive paper), by Dr. William Ogle; on Ruptures of Arteries dependent on external injury, by Mr. G. Pollock; on the Formation of Coagula in the Cerebral Arteries, by Dr. Dickenson; on Talipes Equinus, by Mr. G. Nayler; on Certain Points connected with the Statistics of three hundred Amputations, by Mr. T. Holmes; Statistical Tables from the Dental Case-Books of the Hospital, by Mr. Vasey; and the Annual Reports of Cases admitted into the Medical and Surgical Wards.

XXXIII.—*St. Bartholomew's Hospital Reports*. Edited by Dr. EDWARDS and Mr. CALLENDER. Vol. II. London: Longmans and Co. 1866. 8vo, pp. 264.

This volume contains numerous interesting papers by members of the

medical and surgical staff and former students of St. Bartholomew's Hospital. Among the more noteworthy papers is one by Mr. Savory On the Relation of Phlebitis and Thrombosis to Pyæmia; and another, by Mr. Paget, On Gouty and other forms of Phlebitis.

Mr. Savory sums up his observations in the following words:—

“Thrombosis may exist without any evidence of phlebitis, and very often occurs without being followed by pyæmia.

“Phlebitis may occasionally exist without thrombosis, and often occurs without being followed by pyæmia.

“Pyæmia often exists without any evidence of thrombosis or phlebitis; still oftener it occurs without any evidence whatever that it has been preceded by either of these, or of any other affection of the veins.

“It has therefore not been satisfactorily shown that either phlebitis or thrombosis stands, in any especial or peculiar manner, in relation to pyæmia, as cause and effect.

“Although the ancient idea that the affection termed pyæmia depends on the presence of pus in the blood is no longer generally received, yet there can be little doubt that it still continues to influence largely the views which are taken of its pathology. That the disease is still so commonly associated with what was called phlebitis is due to the old idea that in this way pus is introduced into the circulation. It seems to be a great step onwards to recognise the fact that the disease may occur independently of the presence of pus, or of any affection of the veins whatever—that it is not due to any specific or peculiar matter formed in this or that particular part, but that it is the effect of blood-poisoning; due to the introduction into the circulation of morbid or putrid matter. No doubt pyæmia is often associated with the formation of clots in certain veins, but this coincidence by no means implies the relation of cause and effect. When veins are found occupied by disintegrated clot after death from pyæmia, it has still to be shown that this was the cause of the disease. On the contrary, it can be shown, so far indeed as the most careful and critical examination can go, that pyæmia may occur independently of any affection of the veins whatever. And it is a question whether, in those cases in which the veins are plugged or inflamed, thrombosis and phlebitis are not the local, and pyæmia the general, effect of the same cause.

“No doubt pyæmia is almost always associated with the presence of pus somewhere; but this association does not of necessity imply the relation of cause and effect. The almost constant formation of pus in these cases may be otherwise accounted for, and there is no more evidence of any especial relation between pus and pyæmia than this, that pyæmia is the result of the passage of putrid fluid into the circulation, and that pus is of very common occurrence, and liable, like other animal fluids, to become putrid. It is only when pyæmia is disentangled from any special or specific local affection that there will be any chance of a full and free investigation of its pathology” (p. 60, 6).

Mr. Paget describes a phase of phlebitis not previously recognised. He thinks that the name gouty phlebitis is justified by the number of cases in which phlebitis is associated with ordinary gouty inflammation in the foot, or joints, and occurs with marked gouty constitution, or with gouty inheritance.

“In such cases the phlebitis may have no intrinsic characters by which to distinguish it; yet, not rarely, it has peculiar marks, especially in its symmetry, apparent metastases, and frequent recurrences. Gouty phlebitis

is far more frequent in the lower limbs than in any other part ; but it is not limited to the limb that is, or has been, the seat of ordinary gout. It affects the superficial rather than the deep veins, and oftener occurs in patches, affecting (for example) on one day a short piece of a saphenous vein, and on the next day another separate piece of the same, or a corresponding piece of the opposite vein, or of a femoral vein. It shows herein an evident disposition towards being metastatic and symmetrical ; characters which, I may remark, by the way, are strongly in favour of the belief that the essential and primary disease is not a coagulation of blood, but an inflammation of portions of the venous walls. The inflamed portions of vein usually feel hard, or very firm ; they are painful, aching, and very tender to the touch ; such pain, indeed, often precedes the clearer signs of the phlebitis, and not rarely begins suddenly. The integuments over the affected veins (where they are superficial) are slightly thickened, and often marked with a dusky reddish flush. When superficial veins alone are affected there may be little œdema ; but when venous trunks, as the femoral, the whole limb assumes the characteristics of complete venous obstruction. It becomes big, clumsy, featureless, heavy, and stiff ; its skin is cool and may be pale, but more often it has a partial slight livid tint, with mottlings from small cutaneous veins visibly distended. The limb thus enlarged feels œdematous all through ; but firm, and tight-skinned, not yielding easily to pressure, and not pitting very deeply. By this state almost alone the disease must sometimes be recognised, for it may be very marked when only a small portion of vein is affected, and that (as the lower part of the popliteal) so deeply seated as to be scarcely felt" (p. 83).

The constitutional disturbance associated with this condition is that of slight feverishness, or of an ordinary gouty attack more or less acute. This form of phlebitis, as other forms, may prove fatal by embolism. Mr. Paget believes that the disease is often hereditary. He thinks that gouty phlebitis does not need active treatment. "Leeches," he says, "do no good ; mercury (I think) would do harm, if anything ; purgatives seem unnecessary ; colchicum has the same limited value as in other forms of gout, and appears useful in direct proportion to the severity of the symptoms." Alkaline drinks may be given, the food and stimulants should be diminished, and water-drinking increased. Most important is rest, with the trunk and limbs level, "for in this condition there is the best opportunity for the adhesion of the clot, and its union with the walls of the vein ; and the least risk of detachment." Of local applications, frequent fomentations, and wrappings of the limbs with hot wet flannels, seem best.

XXXIV.—*Sanitary Measures and their Results ; being a sequel to "The History of the Cholera in Exeter in 1832," to which is now added a short account of its occurrence in 1849.* By THOMAS SHAPTER, M.D., Senior Physician to the Devon and Exeter Hospital, &c. Third edition. Exeter : W. Clifford. 1866. 8vo, pp. 36.

The health history of Exeter from 1832 to 1866 in reference to cholera is one of those illustrations of the value of sanitary im-

provements which cannot be too widely known. Dr. Shapter tells the story well, and the publication of a third edition of his useful essay shows that it is not unappreciated.

XXXV.—*The Present State of the Drainage Question considered in its Sanitary, Engineering, Economical, and Agricultural Aspects.* By WILLIAM MENZIES, Deputy-Surveyor of Windsor Forest and Parks. London: Longmans and Co. 1866. 8vo, pp. 49.

In a former work, published in 1865, Mr. Menzies sought to prove that the first principle to be kept in view in all drainage works, either for towns, villages, or single houses, was the entire separation of the rain-water from the proper sewerage, by a system of deep drains for the latter and surface drains for the former.

In this pamphlet Mr. Menzies endeavours to strengthen his argument.

XXXVI.—*On the Future Water Supply of London.* By GEORGE WILLOUGHBY HEMANS, C.E., and RICHARD HASSARD, C.E. London: Edward Stanford. 1866. 8vo, pp. 34.

On the Supply of Water to the Lancashire and Yorkshire Towns, from the Lake Districts of Cumberland and Westmoreland. By THOMAS DALE, C.E., Corporation Waterworks, Hull. London: Kent and Co. 1866. 8vo, pp. 32.

In a previous volume we reviewed an ingenious proposition by Mr. Bateman, C.E., for supplying London with pure water from the Welsh hills. In the present pamphlet Messrs. Hemans and Hassard advance a bolder suggestion, namely, to bring water from the lakes of Westmoreland and Cumberland. This suggestion extends, moreover, to the supply of towns in the intermediate districts. In this scheme the lakes of Haweswater, Ullswater, and Thirlmere would form the reservoir of supply, and these lakes, the rivers Lowther, Eamont, and Greta, and the district draining into them, would be connected to form one great water-service.

"The water supplied to London and the intervening towns," these gentlemen say, "would be taken almost entirely from Ullswater and Thirlmere lakes, and would be of extraordinary purity and excellence, being under two degrees of hardness, and containing, per imperial gallon, not more than about four grains of total impurity, of which scarcely more than half a grain would be organic matter.

"Haweswater and Thirlmere are lonely and unfrequented lakes, occupying deep valleys embosomed in mountains, and afford admirable sites for the construction, at comparatively trifling expense, of immense reservoirs, to which additional supplies of water can with great facility be conducted."

The mode of communication is thus sketched:—

"The water from Thirlmere would be drawn off at its northern extremity, and conveyed to Ullswater by conduit and tunnel; the tunnel would be eight miles in length, but shafts can be put down over the entire distance, and in such a case it is obvious, there would be no greater difficulty in constructing a long tunnel than a short one, it is simply a question of greater length and of additional shafts.

"From Ullswater the supply would be drawn off from the south end of the lake at Patterdale, and from thence carried by tunnel under Kirkstone Pass.

"This tunnel would be the only work of unusual magnitude connected with the project; it would be $7\frac{1}{4}$ miles in length, but of this $5\frac{1}{4}$ miles would be ordinary and rather shallow tunnel, and would therefore present no difficulty; the central portion immediately under Kirkstone Pass would be $1\frac{3}{4}$ miles in length between the shafts, and would, at the rate of progress which has been effected at the Mont Cenis Tunnel, occupy in its construction about three years after the shafts were sunk.

"No doubt, with the rock-boring machines of the present day, it might easily be completed in that or a less period of time.

"From the south end of the tunnel the water would be conveyed to London by conduit, tunnel, and iron pipes; the aqueduct would pass by Ambleside and Kendal, and down the eastern side of Lancashire, avoiding the Wigan Coal-field, to the east of Manchester and of the Potteries district, and to the east of the Staffordshire Coal-field and of Birmingham, and onwards towards London, following a route nearly parallel with that of the London and North-Western Railway, and would terminate in a large regulating reservoir to be constructed to the north of Harrow, at a distance of about 12 miles from Cumberland Gate, Hyde Park.

"The project may, in fact, be briefly described as an aqueduct, or arterial conduit, deriving its supply from the great rainfall and natural reservoirs of the lake country, passing through the heart of England, and capable of affording, *in transitu*, a practically unlimited quantity of the purest possible water to the vast manufacturing districts and population on the line of its route, as well as to the metropolis itself.

"Some portions of the project and of the aqueduct may be carried out in detail, as the demand for water increases from time to time."

Estimates of amount of water available and of cost are given, but the impression left upon the mind is not altogether favourable to the project. It is probable, however, that the mind does not at once grasp the practicability of so bold a conception; and it is not a little heartening to find our civil engineers grappling in anticipation a question which sooner or later must be dealt with in practice differently than at present.

Mr. Dale's proposition appeals much more forcibly to the judgment. It is admirably conceived and argued. He has the priority in looking upon the Lake Districts as available resources of water-supply, but he limits their use to the Lancashire and Yorkshire towns. He finds in those districts, and the character of the water in its lakes, all the requisites for a permanent purity of source, as well as a sufficiency of supply.

"My scheme," he says, "comprehends the obtaining the supplies of water required for the various towns (so situated as to receive them) from the Lake districts of Cumberland and Westmoreland, where such an abundance of the purest water is flowing year after year into the sea, whilst towns with dense populations, within a distance of this source of one hundred miles, are suffering the greatest inconvenience for the want of pure and wholesome water.

"The advantages to be derived from adopting this source are great. Amongst the number, the following may be mentioned:—

"1st. A much higher standard of public health would be obtained.

"2nd. The water, being soft, is better adapted for manufacturing purposes.

"3rd. From its position as a mountainous district, the rainfall is very great; and having large lakes of great natural capacity, forming vast storages of pure water, a certainty of an abundant supply, at all seasons, would be secured.

"4th. That the capital invested in this project would be advantageously expended.

"The high mountains in this district, being exposed to a seaward aspect, secure an immense amount of rainfall, on an area of several hundred square miles. So great a supply of water is far beyond any demands which could be made upon it for towns' supplies."

Of the general outline of his scheme, he says:—

"Amongst the lakes are those of Ullswater and Haweswater. These two lakes, from their altitude, great volume, purity of water, and extensive precipitous area of rainfall, are the best adapted for town supplies.

"Ullswater is about nine miles in length, and varies in breadth from a quarter of a mile to two miles; the north is in Cumberland, the rest of it is in the Manor of Barton, county of Westmoreland. Opposite to Airey Beck it is from 174 to 110 feet deep; at the high end from 60 to 84 feet; and at the lower end—from Old Church downwards—it gradually diminishes from 120 to 36 feet deep. The colourless transparency of the waters of this lake is most remarkable. Out of it issues the river Eamont. The level of its water, as determined by the Ordnance Survey observations at Liverpool, is 477 feet above the mean tidal level, being the datum on which is based the whole of the altitudes of lakes and highest levels of towns.

"Haweswater, although not so large as Ullswater, is very considerable, and has a greater altitude, being 694 feet above the datum mentioned. . . .

"From these two lakes I propose to lay down main trunk pipes.

"The main trunk to consist of several distinct lines of pipes, laid side by side, so that should repairs or contingencies arise, ample provision would be made to retain a constancy of flow.

"At all stations, where branch supplies were required for towns, &c., reservoirs should be constructed to receive the discharge of waters from the mains.

"I propose that the various towns thus to be benefited should combine financially to carry out this project, each bearing its proportion of cost, in proportion to its rateable value or otherwise, as it may deem fit.

"The pipes I propose to lay down have a capacity of a discharge of one hundred and fifty millions of gallons daily; and the waters are proposed to be taken not from Ullswater alone, but also from Haweswater, a lake about three miles long, and from a quarter to half-a-mile in breadth.

"The following towns can be supplied with water discharged into reservoirs by gravitation, as shown by the following table:—

			Highest Level. Feet.		Head of Water. Haweswater.		Head of Water. Ullswater.
Lancaster	132	562	345
Preston	140	554	337
Wigan	157	537	320
Dewsbury	187	507	290
Wakefield	201	493	276
Liverpool	227	467	250
Leeds	300	394	177
Bingley	278	416	199
Kendal	316	378	161
Bolton	327	367	150
Blackburn	373	321	104
Keighley	388	306	—
Bradford	420	274	—
Huddersfield	429	265	—
Burnley	492	232	—
Rochdale	472	222	—
Halifax	604	90	—
Manchester	—	—	—
Salford	—	—	—
Stockport	—	—	—”

We commend Mr. Dale's and Messrs. Hemans' and Hassard's pamphlets to our readers, as important contributions to the solution of a great public question.

XXXVII.—*On the Endoscope as a means for the Diagnosis and Treatment of Urethral Disease.* By CHRISTOPHER HEATH, F.R.C.S., Assistant-Surgeon to, and Lecturer on Anatomy at, the Westminster Hospital. London: J. Churchill and Sons. 1866. 8vo, pp. 22.

This is a brief account of the new method of exploring, by the sight, the urethral, rectal, and vaginal passages. Mr. Heath confines his observations to the use of the endoscope in morbid states of the urethra, first describing the healthy passage as seen through it. Among other observations, he states that he has noticed a constant vermicular contraction of the wall of the canal, apparently passing towards the bladder, and this, he thinks, “accounts for the well-known fact that foreign bodies in the urethra tend to pass in that direction.” We once witnessed the unfortunate accident of a flexible bougie snapping low down in the urethra. A piece of the extremity, a little over two inches in length, remained in the canal just anterior to the membranous portion. Presently, to our surprise and delight, we found that this portion was being gradually forced outwards by the unaided action of the urethra; and in a few moments it was actually thrust out of the passage.

Mr. Heath confirms Desormeaux's observations on the existence of *granular urethritis*, a morbid state first discovered by the aid of the endoscope.

He says that this disease—

“Appears to be of by no means infrequent occurrence in the male urethra, where it keeps up a constant slight discharge—in fact, that slight remnant of gleet, the ‘morning drop,’ which is such a trouble to both the patient and the surgeon. I have been able to detect this condition in various parts of the urethra, sometimes confined to a small spot, and at others spreading for some distance, its most favourite seat being the bulbous portion of the urethra. Here, when the granulations are of large size, they materially narrow the calibre of the canal, giving rise to what may be termed a granular stricture, which is perfectly different in appearance from the true organic stricture, though it is not improbable, as pointed out by Desormeaux, that a long-continued granular condition may lead to the development of a true organic stricture. In fact, this method of the development of stricture has long been known to surgeons, who, in cases of long-standing gleet, always apprehend the formation of a stricture. The appearance of the granulations, even when of small size, is very characteristic; and as they come into view the moment the tube is introduced, it is impossible that they can be due to the irritation of its presence, as has been stated by some observers. The granular condition will be best described by comparing it with that familiarly known as ‘granular lids,’ which the florid granulations cause it closely to resemble; and the treatment is the same—viz., the local application of the strong solution of nitrate of silver I have already mentioned. Not only does this take down the granulations and restore the healthy appearance of the mucous membrane, but *pari passu* the calibre of the strictured portion of the urethra increases; and thus I have frequently found that after a few applications of the solution the largest-sized tube is able to pass where it had previously been prevented, and this without the use of any dilating instruments whatever.”

Mr. Heath reports several cases showing the utility of the endoscope.

XXXVIII.—*Practical Observations on the Intellectual, Sanitary, and Medical Treatment of the Deaf and Dumb.* By HENRY SAMUEL PURDON, M.D., Fellow of the Anthropological Society. Belfast. Sm. 8vo, pp. 95.

This work is devoted to a brief historical sketch of the deaf and dumb; the conditions of the ear, congenital and acquired, observed in mutes, with the various remedies which have been proposed for their cure; the manifestations of disease occurring in deaf-mutes; the diseases to which the deaf and dumb are peculiarly liable; and, finally, the medical and sanitary management and dietetic regimen of these unfortunates.

XXXIX.—*The Nervous System.* By LUDOVIC HIRSCHFELD, M.D., of the Universities of Paris and Warsaw, Professor of Anatomy to the Faculty of Medicine of Warsaw. Edited in English by ALEXANDER MASON MACDOUGAL, F.R.C.S., with artistically coloured lithographic illustrations, designed by J. B. Lèveillé. Part I. J. Churchill and Sons. Imp. 8vo.

Hirschfeld's and Lèveillé's illustrations of the nervous system are unrivalled. Their accuracy, beauty of execution, finish, and delicacy of colour exhaust praise. The republication of these plates with an English text is a happy idea, which we trust will meet with a just appreciation from the profession. The present part contains the plates devoted to the vertebro-cranial dura mater and vertebro-cranial arachnoid.

XL.—*Gleet: its Pathology and Treatment. With a Memoir on the Treatment of Stricture of the Urethra by Subcutaneous Division.* By HENRY DICK, B.A., M.D., Surgeon to the National Orthopædic Hospital. Second Edition. London: R. Hardwicke. 8vo, pp. 113.

Dr. Dick, although using the endoscope, does not appear to assign to it so important a position in the diagnosis of morbid conditions of the urethra as some writers. It may clear up certain doubts, but practically it does not supersede the ordinary methods of investigation. He disbelieves, moreover, in the existence of the so-called granular urethritis. He says:—

“That which is called by modern writers granular inflammation, as seen by the help of the endoscope, is only the swollen and injected state of the vessels of the urethra during chronic inflammation. We can artificially produce the same appearance in the healthy urethra by the introduction of a dilating tube or instrument into the urethra for a short time. On withdrawing the instrument and immediately inspecting with the endoscope, the urethra will be found to present the appearance of granular inflammation. I have chosen sound parts of the urethra and different parts and made those experiments, and always came to the same conclusion—namely, that the granules we see by the help of the endoscope are the swollen vessels, but not new formations as we observe in the uterus or in the eye. The anatomy of the eye and of the uterus is different from that of the urethra. Fibrous tissue or a fibrous membrane is placed in the womb or the eye directly under the mucous membrane. Such is not the case in the male urethra, where a loose vascular tissue forms the subcutaneous tissue of the mucous membrane.”

Dr. Dick describes gleet as existing under three different conditions. 1st. Chronic inflammation of one or several spots, with weakness of the

affected parts. 2ndly. A pathological change of one or several spots of the urethra, with swelling and redness, the affected parts being more or less swollen, contracted, and sometimes deviated. 3rdly. An established structural change of the affected parts.

The suggestion for the treatment of stricture, under certain conditions, by subcutaneous division, is exceedingly ingenious, but for the steps of the operation we must refer to the work itself.

XLI.—*On the Non-identity of the Parasites met with in Favus, Tinea Tonsurans, and Pityriasis Versicolor; including proofs derived from the occurrence of these diseases amongst the Lower Animals, and their transmission from them to Man.* By Dr. M'CALL ANDERSON, Lecturer on Practice of Medicine in Anderson's University, Physician to the Dispensary for Skin Diseases, &c., Glasgow. Glasgow: Wm. Mackenzie. 1866. 8vo, pp. 20.

This is another of Dr. Anderson's instructive contributions to dermatology. He supports his argument of the non-identity of the parasites named by cogent instances, and he appends the following summary of proofs:—

“(1.) In all cases of successful inoculation with the *Achorion*, *Tricophyton*, and *Microsporon furfur*, the same parasitic disease has been produced as that from which the parasite was taken.

“(2.) Of the innumerable cases occurring in the human subject illustrative of the contagious nature of favus, tinea tonsurans, and pityriasis versicolor, which have been recorded, there is no authentic case in which one of these diseases gave rise to one of the others.

“(3.) The difference in the appearance of favus, tinea tonsurans, and pityriasis versicolor, when fully developed, is so very striking as to lead to the belief that they are produced by separate parasites.

“(4.) There is no authentic instance on record of the transition of one of these diseases into one of the others.

“(5.) The difference in the appearance of the achorion, tricophyton, and microsporon furfur is sufficiently striking to enable the observer in many cases to form a correct diagnosis from the microscopic examination alone.

“(6.) Of the numerous instances on record of the transmission of favus and tinea tonsurans from the lower animals by contagion or inoculation, favus has always given rise to favus, and tinea tonsurans to tinea tonsurans.”

Dr. Anderson's references form a useful bibliography of the subject he discusses.

XLII.—*What to Wear in India. An Attempt to apply the Philosophy of Heat to Clothing.* By CAMERON J. F. McDOWALL, Assistant-Surgeon, Bombay Army. Bombay: Thacker. 1865. 8vo, pp. 24.

Mr. McDowall endeavours to instil a better knowledge of the requisites of clothing into his tropical associates. He is brief, to the point, and not inapt in illustration. Thus, writing of colour as influencing the temperature, health, and comfort of the wearer, he says:—

“I remember well that when encamped on the shores of the Bosphorus, in Turkey, a little incident occurred which vividly impressed this physical fact on my memory and shoulders! The forenoon being bright and inviting, we (some other officers and myself) determined, Leander-like, to lave our limbs ‘in the classic waters of the East,’ though not precisely at the same spot that he patronized. The walk was pretty long and hot. We one by one took our coats off and carried them on our arms, both on going and coming from the bath. I wore a cherry-coloured flannel-shirt, and although all our shoulders were more or less burnt by exposure while in the water, I was literally scorched, and for some days suffered greatly from the slightest movement or friction of my coat. We all suffered in exact proportion to the depth of shade of our flannel. Now in the jungle, in the hottest weather and in the sun, I have often ridden, and do ride still, both for experiment and comfort, in my shirt sleeves; but that shirt is white. Any other colour requires a proportioned thickness,—nay, even padding, according to some.”

Mr. McDowall is equal to a joke. “A fanciful idea,” he says in a final note, “suggests itself as to whether the knapsack—which, well-packed, is almost bullet proof—could not be so made as to be worn temporarily in front of the chest *during action*. The ordinary bayonet thrust could not pierce in. Certainly at present during battle it is only an encumbrance. I claim the patent of this *pièce de résistance*.” This is not bad for the tropics.

XLIII.—*Fecundity, Fertility, Sterility, and Allied Topics.*

By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.S., Lecturer on Midwifery to Surgeons’ Hall Medical School, Physician for Diseases of Women to the Royal Infirmary, &c. Edinburgh: Adam and Charles Black. 1866. 8vo, pp. 378.

This is the most important work on the intimate laws affecting population which has issued from the press for some time. It contains a mass of facts and deductions which almost set abstraction at defiance. We shall simply bring together some of the most important of Dr. Duncan’s deductions.

On the fertility (productiveness) of the female population at different ages, he concludes that the actual, not the relative, fertility of our female population, as a whole at different ages, increases from the commencement of the child-bearing period of life, until the age of thirty is reached, and

then declines to its extinction with the child-bearing faculty. Further, that actual fertility is much greater before the climax, thirty years, is reached, than after it has passed. And, finally, that at least three-fourths of the population are recruited from women not exceeding thirty years of age.

The comparative fertility of the whole female population at different ages increases gradually from the commencement of the child-bearing period of life until about the age of thirty years is reached, and then it still more gradually declines. It is greater in the decade of years following the climax of about thirty years than in the decade of years preceding the climax.

The fecundity (capability to bear) of the mass of wives, it would appear, is greatest at the commencement of the child-bearing period of life, and after that period gradually declines. The fecundity of the whole wives in our population included within the child-bearing period of life is, before thirty years of age is reached, more than twice as great as it is after that period. The fecundity of wives in our population declines with great rapidity after the age of forty years.

The initial fecundity of women gradually waxes to a climax and then wanes; it is very high from twenty to thirty-four years of age; and the climax is probably about the age of twenty-five years.

Nearly all women married at from twenty to twenty-five years of age are fecund; and the fecundity of very young (fifteen to twenty years) wives, below twenty, is greater than that of wives married at from twenty-five to twenty-nine.

Passing over the questions as to the weight and length of the newly-born child, and the influence of age of the mother and primogeniture upon the infant, we come to the questions relative to twins, an examination of which leads to the following conclusions:—

“1. The largest number of twins is produced by women from twenty-five to twenty-eight years of age; and on each side of this climax of fertility in twins there is a gradually increasing falling off in their number as age diminishes on the one side and increases on the other.

“2. Twins are not regularly distributed among births generally; their production, therefore, is not subjected to the same laws as govern ordinary fertility.

“3. The mean age of twin-bearing mothers is greater than of mothers generally.

“4. Twins increase in frequency as mothers become older. This forms a striking contrast to the fecundity of a mass of wives (not mothers) which diminishes as their age increases. It accords, however, with the law of intensity of fertility of fertile women.

“5. Newly-married women are more likely to have twins the older they are.

“6. While the fecundity of the average individual increases with age till twenty-five is reached, and then gradually diminishes, there is some probability that the opposite is true, so far as regards twins alone, fertility in twins being greatest when fecundity is least, and *vice versa*.

“7. The actual number of twins born of a mass of women in different pregnancies decreases as the number of pregnancies increases.

“8. The number of twins, relatively to the number of children born in different pregnancies, increases with the number of the pregnancy. In other words, a woman is more likely to have twins in each succeeding pregnancy than in the former pregnancy. The first pregnancy forms an exception to this rule.

"9. In an individual, twin-bearing is a sign of high fertility at the time. It also, in a mass of women, shows a high amount of fertility, at least till the time of the birth of the twins.

"10. It is probable, though not proved, that twin-bearing women have larger families."

The laws of the fertility of women are next discussed in their different bearings; and then Dr. Duncan enters upon the consideration of the laws of sterility. Some of the deductions in the latter section will indicate the interesting matter to be found herein. Thus, we learn that relative sterility—that is, the condition of a woman who, while she may or may not be sterile, is, under ordinarily favourable circumstances for breeding, sterile in relation to the circumstances of time—will arrive after a shorter time, according as the age at marriage is greater. The older a fertile woman is at marriage, the older is she before her fertility is exhausted; that is, before the advent of relative sterility. A wife who, having had children, has ceased for three years to exhibit fertility, has probably become relatively sterile; that is, will probably bear no more children; and the probability increases as time elapses.

An examination of the questions connected with the mortality of child-bed leads to several important deductions or observations, of which not the least is, "that the age of greatest safety in parturition concludes with the age of greatest fecundity, and that during the whole of child-bearing life safety in parturition appears to be directly as fecundity, and *vice versa*."

The age of fertility is considered by Dr. Duncan, and then he enters upon the questions connected with the duration of labour, the interval between insemination and conception, and insemination and parturition; also the interval between the last menstruation and parturition, the prediction of the day of confinement, and the protraction of the period of pregnancy. We shall merely whet the minds of our readers with this summary of the contents of the latter portion of Dr. Duncan's work, for (as, indeed, is to be said of the whole of the book) justice only can be done to it by reading it.

Dr. Duncan's work will find a permanent place upon the bookshelves of all thoughtful men who take an interest in the great questions of the propagation of our species.

XLIV.—*Diarrhœa and Cholera: their Nature, Origin, and Treatment through the Agency of the Nervous System.*
By JOHN CHAPMAN, M.D., M.R.C.P., M.R.C.S. Second edition, enlarged. London: Trübner and Co. 1866. 8vo, pp. 248.

It is fortunate that Dr. Chapman has himself summed up his neurophysiological positions, or we might run the risk of being charged with mutilating his theory. He starts from the following propositions:—

"1. That the chief function of the sympathetic nervous system consists in regulating the diameters of the blood-vessels throughout the body.

"2. That when the sympathetic ganglia are in a state of maximum hyperæmia the nervous effluence from them to the muscular coats of the arteries to which they are severally related stimulates them so excessively as to induce in them a condition of tonic spasm—a spasm so intense as to result in shutting off the blood altogether from a large proportion of the peripheral arteries.

"3. That when the sympathetic ganglia are in a state of maximum anæmia the nervous effluence from them to the muscular coats of the arteries to which they are severally related becomes so extremely feeble that a condition resembling paralysis is induced; the muscular coats of the arteries become consequently extremely relaxed; and, as the blood flows in the direction of least resistance, the parts supplied by the arteries in question become suffused with blood to an excessive degree.

"4. That when the spinal cord is in a state of hyperæmia, cramps of the involuntary muscles surrounding the alimentary tube as well as cramps, or even convulsions of the voluntary muscles, which are due to such hyperæmia, are likely to ensue.

"5. That every gland and glandular follicle in the body is under the control of one motor nerve (which I call the *positive motor*) emerging from the cerebro-spinal system, and distributed to its secreting cells in order to regulate its functional activity; and of another motor nerve (which I call the *negative motor*) emerging from the sympathetic system, and distributed to its artery or arterial twig, in order to regulate its blood-supply.

"6. That in the same manner as glands are supplied with positive, as well as with negative, motor nerves, so, there is reason to believe, every tissue of the body is thus supplied, and is thus placed and sustained in a state of elective affinity for the elements of the blood requisite for its nourishment and functions.

"7. That the sympathetic ganglia and the spinal cord can be rendered hyperæmic or anæmic, artificially, by means of heat, in the one case, and cold in the other, applied along the spine.

"8. That by means of heat applied along the spine the general circulation may be lessened, the activity of the glandular system may be increased, and, in some cases, cramps of both the voluntary and involuntary muscles may be induced.

"9. That by means of cold applied along the spine the general circulation may be increased, the activity of the glandular system lessened, and cramps of both voluntary and involuntary muscles may be arrested or prevented."

It will be observed that there is a question here of theory within theory; and in the subsequent portions of Dr. Chapman's work, when the etiology of diarrhœa and cholera is discussed, other subsidiary theories arise out of the former, until the unfortunate reader is left in bewilderment. It may be questioned even if the writer himself has escaped from the confusing tendency of his own active speculations. Thus in the "preface" there are a series of suggestions about electricity as a probable efficient cause in the production of cholera, which leave all sober reasoning and modest facts in the infinite distance. "There may already exist," writes Dr. Chapman, "for aught I know, in astronomical observations as elsewhere, records of observations of electric phenomena, both atmospheric and telluric, during cholera times, sufficient at all events to admit of the institution of a provisional parallel between the various degrees of

intensity of the cholera epidemic and of electric disturbances in the different localities in which the disease has prevailed" (p. viii). If this means anything at all, it means that Dr. Chapman prefers to speculate upon certain problems of physical and physiological science without the restraint of facts, for such only can be the conclusion attached to an indisposition to take the measures to ascertain whether any systematic observations exist bearing upon the question at issue. The promiscuous facts and statements he quotes to show *some* possible connexion between the electrical state of the atmosphere and disease are worthless as ballast for his prefatorial speculations.

The principal interest of this edition of Dr. Chapman's work exists in the chapter on the results of treatment. If clinical experience shows that the treatment of diarrhoea and cholera by the spinal ice-bag gives satisfactory results, we will take the facts irrespective of the theory. It has unfortunately happened that Dr. Chapman's opportunities of trying his plan have been few. The apparent positive results obtained by the application of the ice-bag in collapse have been the promotion of a more steady reaction and the relief of cramps. One or two of the cases recorded seem to leave little doubt in these respects, and these facts command attention.

The general results of the treatment, as applied at Southampton, are thus stated:—

"So far as I am aware, the only medical men in Southampton who, during the present cholera epidemic, have had any patients treated by means of modifying the temperature of the spinal region, are Mr. Bencraft, and Drs. Griffin, Cheeseman, and Olliver.

"The aggregate number of cholera patients treated by them up to the 20th of July was 72: of these, 44 were submitted to ordinary treatment; 25 had died before I left Southampton on the 21st instant, and of the 19 survivors, some had recovered, and some were recovering. Of the 28 who had been treated by means of ice, 13 had died before the 13th instant; and of the 15 survivors, some had recovered and some were recovering.

"According to these statements, it appears that whereas only 43 per cent. of those patients submitted to ordinary treatment have recovered, 53 per cent. of those treated by means of ice have been saved. But it will be observed that in Case 25 the child was moribund when the ice was applied; that in Case 31 the ice was not kept along the spine; and that in Cases 35 and 36, owing to the ignorance or stupidity of the attendants, the patients were drenched with iced-water, which escaped from the unfastened bags. These four cases, therefore, cannot be held to have been cases in which my method of treatment was tried at all. If these be excluded, then of those treated 60·2 per cent. had recovered, or were recovering, when I left Southampton.

"But this comparison of the aggregate results of the two methods of treatment is far from showing the full remedial power of my method of treating cholera, as compared with the others adopted in Southampton. Of the whole 23 patients treated by Mr. Bencraft, the only four who survive were treated by ice. Again, if Dr. Griffin's patients be ranged into three classes of cases, viz., choleric, slight collapse, and complete collapse, as he has carefully distinguished them, it will be found that there are 10 of the first, 5 of the second, and 7 of the third class. Now, as all the patients treated by him, or in conjunction with me, by means of ice, were of the second and third class only, it is necessary, in order to institute a correct com-

parison of the two methods, to ignore the 10 cases of cholérine, and to compare the results of treatment in respect only to the cases of slight and complete collapse. The results are as follows :—

Cases under ordinary treatment.

Slight collapse	5	all recovered.
Complete collapse	7	all dead.

Cases under Ice treatment.

Slight collapse	3	all recovered.
Complete collapse	10	of whom 5 recovered.

"It thus appears that while *all* the cases of complete collapse, viz., 14, which were submitted to ordinary treatment by Mr. Bencraft and Dr. Griffin, *died*, 4 of Mr. Bencraft's cases, and 5, or the half of Dr. Griffin's cases, altogether 9 out of 22, or 40 per cent. of those treated by means of ice, recovered.

"The results would have been much more favourable still if all the cases treated had been treated properly. For example, in Case 27, death was most probably induced by the improper use of the hot-water bag ; Case 30 was only seen once, and can scarcely be said to have been treated at all. Moreover, whereas an essential part of my method of treatment consists in applying heat thoroughly and constantly to the general surface of the body in all severe cases, while the ice-bag is being applied along the spine ; in Cases 26 and 33 this was not done at all ; and in Cases 29, 30, 32, 34, it was done but very partially and inadequately. Nevertheless, besides the two fatal cases in which complete reaction from collapse was obtained, in Cases 26 and 29 the improvement effected was so considerable as to assure me that they would have recovered had they been in all respects properly treated ; and even in Cases 32 and 34, so much reaction was obtained as to forbid despair while there is life, if only the treatment in question be thoroughly and energetically persisted in.

"Every one who knows how powerless medicine is to rescue patients from collapse, must admit that the results here described deserve the earnest attention of all interested (and who is not ?) in finding a successful remedy for cholera ; for it appears that out of the 24 cases treated, of which 22 were in collapse, 15 were saved, 2 were completely recovered from collapse, 2 were rallied to a great extent, and 2 in a lesser degree, notwithstanding the partial and ineffectual way in which the treatment was tried.

"As already shown, if the whole 28 cases nominally treated by ice be considered, the proportion of recoveries is 53 per cent. But if the four which were really not treated be excluded, then the proportion of recoveries is 60 per cent. (or 2 per cent. less than that of the recoveries by means of ice at Southampton, in 1865), under all the disadvantages of inadequate trial already described. Though at the time when I last left Southampton the proportion of recoveries under ordinary treatment was, as already stated, 43 per cent., the results as published in the *Medical Times* of August 4th, 1866, show a still less proportion of recoveries under that treatment. Up to July the 31st there had been 158 cases, and 101 deaths : now, after the 28 cases treated by ice, 13 of which were fatal, are deducted from these numbers, it appears that 130 cases have been submitted to ordinary treatment, and that 88 of them have proved fatal, thus leaving 42 patients, or 30 per cent., who had recovered or were recovering. Concisely stated, the results of my method of treatment, as compared with the others pursued at Southampton, are as follows :—*Nearly two-thirds of the patients treated by ice recovered ; more than two-thirds of the patients submitted to ordinary treatment died.*"

The fatal objection to the results here given is the smallness in number of the cases. It is no fault of Dr. Chapman that his field of observation has been so limited, and apart from all theory, the results obtained at Southampton show that, in a disease like cholera, when in the advanced stages, medicine has hitherto been at fault, we cannot afford to throw aside a suggestion supported even by so small a number of cases as Dr. Chapman records. Should cholera again reappear among us, this practical question here at issue should not be left again in doubt.

XLV.—*The Cholera Map of Ireland: with Observations.*

By Sir J. DOMINIC CORRIGAN, Bart., Physician in Ordinary to the Queen in Ireland. Dublin: Browne and Nolan. 1866. Sm. 8vo, pp. 18.

Sir J. D. Corrigan republishes the cholera-map of Ireland for 1848-50. His object is to show that contagion, admitting it to exist in cholera, is an element in the spread of the disease less to be dreaded than the contagion of small-pox, typhus fever, follicular or typhoid fever, erysipelas, measles, or scarlatina, and that therefore there exists no good reason for considering cholera, in regard to contagion, in any light different from other epidemic diseases.

Of the significance of the map in relation to this view we quote Sir J. D. Corrigan's own words, more particularly as they recall some interesting facts in the history of the epidemic of 1848-49 in Ireland:—

“It was naturally to be expected,” he says, “if contagion promoted by population, free intercourse, and the bringing of numbers together in commerce, trade, and manufactures, were an element of much power in propagating the disease, that the map would show that the greater number of towns attacked were in those parts of Ireland where trade, manufactures, and frequent intercourse brought multitudes together; but the contrary is shown by the map, for in the whole of Connaught, and a considerable portion of Munster—its western portion—not a town escaped; while in the other provinces of Ireland, Ulster, Leinster, and the eastern parts of Munster, where trade, manufactures, and commerce brought much greater numbers together, the red dots are considerable in proportion, showing the number of towns that escaped.

“This is just the reverse of what would have occurred was contagion an element of power in propagating the disease. Connaught, with comparatively little intercourse on its seaboard with England and other countries, with a scattered population, without a single manufacturing town, does not show a red mark—not a town escaped. A similar observation applies to the western half of Munster; while the north of Ireland, with its teeming population, its manufacturing towns, bringing great numbers together of all ages, its seaports and factories, shows a large proportion of red-dotted towns. This observation also applies to nearly the whole eastern half of Ireland; so that if we divide the map, by a line extending through the centre of Ireland, from north to south, we arrive at this result—that the western half, with a scattered population, with little traffic, with little direct

intercourse with other countries, without factories to bring numbers together—in fact, with the least means of conveying contagion—does not show more than five towns free from the visitation ; while the eastern half of Ireland, that on the right-hand side of the line, with its populous cities and towns, its numerous seaports, roads, and factories, presenting all the facilities for the conveyance of contagion, shows a very large proportion of red-dotted towns, or towns that did not suffer from the visitation. The numbers will stand thus in provinces :—

“ULSTER.—34 towns of 2000 inhabitants and upwards ; of these, 17, or exactly one-half, escaped the visitation of cholera.

“LEINSTER.—41 towns of 2000 inhabitants and upwards, of which 9 escaped ; 32 were visited by cholera.

“MUNSTER.—47 towns, of which 5 escaped and 42 suffered.

“CONNAUGHT.—14 towns, of which not one escaped.

“It appears to me impossible to look at the map and these returns, without admitting the conclusion, that whether cholera may or may not become contagious, the element of contagion is of comparatively little importance ; and that our best protection will be, not in devoting too much of our energies to combat this element, but in improving the safeguards or protective powers that will render the frames of the people able to resist the influences that spread the disease, telluric, atmospheric, or contagious, as they may be.”

Sir J. D. Corrigan supports his views relative to the contagion of cholera by the expressed opinions of the Irish Health Commissioners for 1849, and particularly by the opinions of Dr. Gavin Milroy, Dr. John Sutherland, and others in the Report of the Council of the Epidemiological Society on Cholera Hospitals, published in 1866.

He sums up his practical opinions in the following terms :—

“1. That opinion is divided as to whether cholera is or is not contagious.

“2. That its contagious nature has never been demonstrated ; the proofs in support of its contagious character never amounting to more than probability.

“3. That its outbreak and its zones in India demonstrate, to a certainty, its capability of arising without contagion, and probably from telluric influence.

“4. That it is admitted by all, even the most ardent contagionists, that if it be contagious, its contagious character is less than that of typhus and typhoid fever, of scarlatina, small-pox, measles, &c.

“5. That assuming this, there exist no good grounds for excluding cholera patients from hospitals adapted for the reception of other epidemic or contagious diseases.

“6. That the shutting of such hospitals against them has an injurious effect on the whole mass of the people, both in regard to health and moral feeling.

“7. That inculcating a great fear of contagion tends to demoralize the people in their social relations.

“8. That we should inculcate these two principles—

“That, even admitting cholera to be contagious, it is less so than other epidemic diseases with which the people have been long familiar ; and that their best power of resisting it, whether depending on atmospheric or telluric influence, or on either or both combined with contagion, is by maintaining mind and body in the most perfect condition of health.”

This pamphlet is an interesting contribution to authoritative opinion concerning the subject to which it refers.

XLVI.—*The Law relating to Public Health and Local Government, including the Law relating to the removal of Nuisances injurious to Health, the Prevention of Diseases, and Sewer Authorities, with the Statutes and Cases.* By W. CUNNINGHAM GLEN, Esq., Barrister-at-Law. Fourth edition. London: Butterworths, and Knight and Co. 1866. 8vo, pp. 1000.

This work deals with the laws of public health so far as they affect local authorities. It contains in fact, to use the author's words, "the whole of the law of 'Local Self-Government,' in relation to the conservation of the Public Health, and to all those numerous matters upon which life, health, and property depend." It does not deal, except in brief reference, with the Acts relating to bodily care, such as the Factory and Mine Acts, the Acts concerning Adulteration of Food, Vaccination, Contagious Diseases, &c., as these Acts do not throw any responsibility upon the local authorities.

This being understood, the present work is the most comprehensive we possess as an aid to a right comprehension of the laws to which it refers. The legislation of 1865 and 1866—in the former of which years the Sewage Utilization Act was passed, in the latter the Sanitary Act—rendered a revision of the work necessary. This has been done, and the arrangement of the book may be thus briefly sketched. It is divided into six parts. The first part treats of the formation of the districts of the local boards, the constitution of those boards, their election and general powers; the second part, of their powers as to sanitary matters and the local government of towns; the third part, of the powers of local boards as to rating, raising money on mortgage of the rates, purchase of lands, audit of accounts, contracts, arbitration, legal proceedings, bye-laws, and other miscellaneous subjects; the fourth part, of the removal of nuisances injurious to health; the fifth part, of sewage utilization, and the constitution of sewer authorities under the Acts 1865 and 1866; and the sixth part, of the law relating to the prevention of epidemic, endemic, and contagious diseases.

It may be useful to mention that Mr. Glen's work includes the Quarantine Act.

XLVII.—*The True and the False Sciences: a Letter on Homœopathy.* London: J. Churchill and Sons. 1866. 8vo, pp. 40.

A vigorous application of good argument to a bad subject.

XLVIII.—*An Essay on the Life in Nature.* By LOUIS MACKALL, M.D. Washington. 1865. 8vo, pp. 12.

Extract from an Unpublished Essay on Physical Force. By the same author. Washington. 1865. 8vo, pp. 34.

An Essay on the Law of Muscular Action. By the same author. Second edition, revised. Washington. 1865. 8vo, pp. 34.

Dr. Mackall delights in investigating the recondite problems of life and physical force. His observations on the law of muscular action were guided by a painful event in which all will sympathize. His own statement will best show the tendency of his researches and the circumstances under which they were undertaken:—

“For the last thirty odd years,” he says, “I have been engaged in investigating this subject of muscular action, but have reasoned solely from instances derived from vital phenomena, or such as are observable in the living body. The following is a succinct account of my proceedings in this investigation:—

“From often observing in the practice of medicine, the inadequacy of remedial means made use of, and from frequently experiencing disappointment in my anticipation of the results of the operation of such means, I became convinced that there was something wrong—some great error in the theory, or in the principles of medicine in which I had been taught.

“This conviction was brought forcibly home to my mind by the death of my wife from uterine hæmorrhage, notwithstanding the use of all the remedial means known to myself and a very skilful medical friend who was present.

“Under the influence of grief consequent on this bereavement, I resolved that I would devote the residue of my life to the task of endeavouring to discover, if possible, the error in the theory of medicine that I had before suspected, and in which suspicion I was confirmed by my late experience.

“While studying this theory in the books for the above purpose, a case of whitlow (*paronychia*) was presented for treatment. Prompted by the resolve mentioned, I carefully noted the prominent facts of the case. I particularly noticed the pulsation of the arteries at the diseased point and at the wrist; and my attention was forcibly arrested by observing the remarkable difference in that pulsation at the two points. That in the finger was full and strong, while the pulsation of the artery at the wrist was comparatively calm.

“Reflecting on the above fact, I arrived at the conclusion that there must be some agency in the arteries of the finger, that were throbbing so violently, to produce this result, that was independent of that in the general circulation; and, in casting about in my mind for some suggestion as to what that agency could be, it occurred to me that the throbbing or distension of the arteries was occasioned by the *action* of their muscular fibres. The correctness of this explanation of the phenomenon in question was confirmed by running over in my mind, as I did at the moment, a number of instances wherein irritation was attended with the distension or dilatation of the tubes

or hollow organs, when such organs were supplied with muscular fibres—as in the œsophagus, in the stomach, intestines, in the uterus in pregnancy, &c.

“I had now arrived at a definite proposition; and in the year 1834, in November, I wrote down that proposition in the following words: ‘All the tubes of the animal body, which are supplied with muscular fibres, have their calibres increased by the *action* of those fibres.’ This was shown and explained to four medical gentlemen at the time mentioned, and signed by them, witnessing that it was so shown. Three of these gentlemen, namely, Drs. J. H. Skinner, B. B. Hodges, and William Ghiselin are now (1865) living. One of them, the late Dr. Henry Brooke, died a few years since.

“In 1836 I forwarded a paper, setting forth the above idea by an application of it to a number of vital phenomena, to the Professor of Anatomy in the University of Maryland, requesting him to advise me as to the best mode of bringing the subject to the notice of the medical profession. My communication was treated with contempt, as were also several papers written on the same subject, and shown to members of the medical profession.

“Although I was fully convinced, from the period mentioned above, that the calibres of the tubes and hollow organs were increased by the action of their muscular fibres, I did not fully comprehend how this occurred until the spring of 1842. At that time, being in conversation with a gentleman who was fond of gesticulating, he, in derision of something that was said, thrust out his tongue over his under lip. This sudden elongation of the tongue instantly suggested that which I had been in search of for eight years—a rational explanation of the action of the fibres about the tubes. The truth flashed on my mind, *that the fibres of muscles are actively elongated by innervation.*”

Dr. Mackall's essays on “Physical Force” and “Life in Nature” deal with abstract questions.

XLIX.—*On the Treatment of Lupus.* By J. L. MILTON, Surgeon to St. John's Hospital for Diseases of the Skin. London: Robert Hardwicke. 1866. 8vo, pp. 27.

Mr. Milton discusses the different remedies used in this formidable disease. He holds to be of doubtful value barium, antimony, soda, cod-liver oil; “most of those preparations of iodine, mercury, and potass, which are not absolutely useless, particularly alterative doses of mercury, or combinations of it with iodine and arsenic, as in Donovan's solution.” As remedies of no value at all Mr. Milton regards steel, quinine, animal acids, bitters, opium, wine and beer, change of air and sea-bathing.

Again, external remedies are divided by Mr. Milton into two classes—the useless and the hurtful. Under the head of useless he includes most of those not strong enough to give pain, while nearly all those strong enough to give pain, he thinks, might safely be classed as hurtful.

The remedies which Mr. Milton proposes himself are much the same as others use—namely, arsenic, mercury, and sometimes iodine; the difference lies in the mode of giving them; “but this difference,” he

says, "is just the most important feature in the whole matter; and one to which I was led, not by any desire to introduce a novelty, but my utter failure with ordinary methods."

Arsenic, he advises, should be given in simple solution and uncombined with any other preparation. It should, as usual, invariably be taken immediately after meals, or with food, but in a quantity always *sufficient to produce some disorder of the stomach*. Till this is done, arsenic, according to Mr. Milton, is powerless against the complaint. Experience alone in the individual case can determine the dose:—

"The only safe plan then, that I know of," he writes, "is to begin with five-minim doses three times a day. If at the end of a few days this quantity brings on sickness or nausea, coated state of the tongue, headache, or great languor, the dose is quite large enough, and instead of being increased may possibly require to be lessened after a few days. But if nothing of this kind occurs, or if such symptoms have occurred and have been subdued, double the amount may be ordered and continued steadily for a few days to see whether any disturbance of the system sets in—if so, no further increase will be requisite for a little time at any rate, or even a reduction may again be called for; but if not, fifteen minims three times daily may be given with the utmost confidence. With proper care, nothing is likely to ensue beyond trifling sickness."

Under circumstances of health, when the tongue becomes coated and the system is intolerant to arsenic, calomel, Mr. Milton holds, will best tend to check the progress of lupus, and improve the general health. But he notes that it is absolutely necessary that the calomel should act as a purgative. Anything short of this he believes to be useless. At first he gives a small dose, say a grain, twice a week. This is usually sufficient. When this dose fails the quantity must be increased and the action aided by an aperient.

The use of iodide of potassium Mr. Milton restricts to cases of lupus confined to the limbs.

Of outward applications Mr. Milton says:—

"I believe that their principal value is restricted to excluding the air, and that those are the best caustics which effect this most certainly and with the least pain. Perhaps the nitrates achieve this result more certainly than any other means. When the patient can remain indoors, and does not care about the dark stains caused by it, the nitrate of silver may be used;* it is an excellent remedy either solid or in saturated solution. In the lupus of children, even a very weak solution can scarcely be borne. Here it is not a bad plan to use a solution of sulphate of copper† for some little time till the sensibility has become rather deadened. The acid nitrate of mercury is a very valuable preparation, and has the advantage of not forming so dark a crust; it is peculiarly suited for small, not very sensitive ulcers and tubercles. It may be brushed with a glass brush over the part, and should be used at first diluted with water till the full strength can be borne. When applied, a basin of water should always be at hand, and so soon as ever the pain begins to be felt, the surface should be freely washed. The yellow nitrate of mercury may also be used in the form

* It was a favourite remedy with Rayer.—*Theoretical and Practical Treatise*, p. 681.

† R Cupri sulph. gr. vj. ; aquæ rosæ ℥ij.

of ointment made with the lard as prepared by Mr. Squire ; it is chiefly adapted to those cases where there is only slight or superficial ulceration, and to the lupoid form of syphilis ; it answers very well for those patients who cannot well have anything applied which produces a visible mark."

Mr. Milton adds much excellent advice upon diet, states that the surface affected with lupus should not on any consideration be touched with cold water, and terminates his pamphlet with the reports of several cases.

L.—*On the Rational Employment of Mercury in the Treatment of Syphilis.* By Dr. COLOMIATI MEREDYTH. London : Robert Hardwicke. 1866. 8vo, pp. 41.

This is a temperate summing up of the argument in favour of the use of mercury in the treatment of syphilis. Dr. Meredyth gives an interesting sketch of the introduction of mercury into the treatment of the disease. He shows the limitations to its use which a progressively increasing knowledge of syphilis has brought about, and satisfactorily elucidates the error which underlies the reasoning of the anti-mercurialists. The pamphlet will be read with profit by all who take an interest in the discussion.

LI.—LXIII.—PAMPHLETS ON CHOLERA.

Epidemic Cholera and Epidemic Diarrhœa: Can these Diseases be Prevented? Practical Suggestions for diminishing the Severity of and Mortality from these Epidemics. By Dr. CAMPS, M.D., M.R.C.P., &c. London : H. K. Lewis. 1866. 8vo, pp. 7.

Dr. Camps proposes a scheme of medicinal prophylaxis against cholera. Its application is best stated in his own words :—

"The great pathological problem to be solved, the beneficial sanitary result to be obtained, in this condition of the entire community is this :—How, or by what means can the population of our towns and country districts be placed in such circumstances, as that they shall not morbidly succumb to the pernicious influences surrounding them ? I entertain a strong persuasion, almost amounting to conviction, that this may be done in the mode now indicated ; in other words, that we possess the means to accomplish this most desirable end. And this, too, mainly and medicinally, by the internal administration of *quinine* and its salts. I would have our families, wherever threatened with an attack of Epidemic Cholera, or of Epidemic Diarrhœa, so placed under the influence of *quinine*, as to become *quinidized* or *cinchonized*, or, they might be *salicinized*, or *arsenicized*, by the administration either of *salicine*, or of *arsenic*. The mode herein indicated, aims at the diminishing, or even at the destroying, of the predis-

position to take these diseases; without which they can take no powerful hold upon the animal organism.

"I would, therefore, most strongly recommend that all persons employed in any way as attendants upon or about those attacked with epidemic cholera or diarrhœa should not fail to take from time to time repeated doses either of *quinine* or of *salicine*, with the view to protect themselves from attacks of these diseases, by diminishing or possibly by destroying altogether any predisposition they may have to fall victims to their influence when exposed thereto. The class of persons here referred to need not be specially indicated; every description of attendants upon the sick being included in this category, as friends, nurses, sick visitors, &c. &c."

Examen Critique des Diverses Opinions sur la Contagion du Choléra. Par le Docteur STANSKI, Ancien Interne des Hôpitaux de Paris. Paris: J. B. Baillière et Fils. 8vo, pp. 144.

Dr. Stanski finds himself in a difficulty at the commencement of his argument. In dealing with the hypothesis of the contagion of cholera, he finds himself about to fight a shadow. Not a solitary fact do his opponents advance with which he can grapple. They cannot produce any peremptory proof either of choleraic germ, miasm, ferment, principle, and so forth. Their sole argument is *post hoc, ergo propter hoc*. Like the circle-squarers and the perpetual-movement mongers, they cry out, prove that our view is not the true one. Nevertheless, the question they have raised narrowly touches the comfort and interests of humanity. Logical quibbles and errors of reasoning must not, therefore, deter the thoughtful from investigating it. Dr. Stanski, therefore, addresses himself to the task, and much to his own satisfaction endeavours to show the folly of the belief of cholera being a contagious disease.

Doctrine Stœchiologique du Choléra d'après les travaux récents. Par A. PAPIN-RUILLIER-BEAUFOND, D.M. Paris: G. Baillière. 1866. 8vo, pp. 64.

Dr. Beaufond seeks to unfold the ultimate morbid change—the stœchiological modification—which gives rise to cholera. He seeks to show that the disease is a general malady, belonging to the class of virulent disorders and of miasmatic origin. The first method of comprehending the nature of such a malady, he holds, is to study the changes determined by the malady in the anatomical elements and fluids of the body, to seize upon the immediate poisonous principles, to determine the nature of the new property they have acquired, and in what manner these new properties are incompatible with life.

Du Choléra Asiatique comme Conséquence d'un Élément Morbide de Nature Organisée. Par M. FAUCONNET, Officier de Santé. Paris: E. Savy. 1866. 8vo, pp. 64.

M. Fauconnet endeavours to show that the morbi-genetic principle of cholera is an animalcular principle of the nature of a ferment. This principle is susceptible of transportation by the atmosphere, short, but

successive distances, and by vessels, either through men or merchandise, to greater distances, after the fashion of those animalcules in a larval or perfect state, like the diminutive diaphanous butterflies, invisible to the eye. It follows that the propagation of these animalcules, in the localities where they are transported, would be brought about most fully where the sanitary condition of the places was worst. Also it is easy to conceive how the common means of prevention by domestic and public sanitary measures would be fatal to their existence. Finally, Dr. Fauconnet maintains, the direction of search for medical treatment is in the discovery of such means as will destroy this animalcular life without injuring the sick.

Le Choléra est-il Contagieux? By le D. HALMAGRAND. Orleans : Vaudecrainè. 1866. Sm. 8vo, pp. 32.

Dr. Halmagrand endeavours to show that cholera cannot be communicated either by contact, the dejections whether from the stomach or the bowels, the emanations of the patient, or by inoculation with his blood. He believes that the cause of the disease is to be found in the atmosphere and on the surface of some localities, and that it possesses a power of spontaneous migration.

Suggestions in reference to the Present Cholera Epidemic, for the Purification of Water Supply, and the Reclamation of East London. With Remarks on the Origin of Cholera Poison: Proofs given in facts connected with the Sources of Water Supply in India. With illustrative incidents descriptive of successful preventive measures and curative efforts. By WILLIAM SANDERSON, C.E. London : Wm. Macintosh. 1866. 8vo, pp. 31.

We fear that Mr. Sanderson's speculations on the origin of the cholera poison will command less respect than his facts relative to the connexion of cholera-outbreaks in India with the state of the water-supply. These latter may be read with profit and interest.

The Human Blight and Cattle Blight; or, an Explanation of the Cholera and Cattle Plague. London : Longman and Co. 1866. 8vo, pp. 30.

The author of this pamphlet must state his own views :—

“From the regions of the East there settle upon our land, every now and then, what are called ‘blights.’

“These blights consist of animalculæ.

“In the case of the vegetable world, it is clear that these ‘blights’ come to destroy different plants. One blight settles upon all the apple trees, wholly passing by the other trees of the garden ; another blight descends upon the currant bushes ; another upon vines, and so on.

“Each of these consists of insects expressly formed for the purpose of settling upon and devouring a particular kind of vegetation only, and which finds no sustenance in any other.

"The cattle plague is caused by a blight of insects expressly formed for the purpose of settling upon and devouring the mucus of the intestines, and ultimately the intestines, of cattle.

"The cholera is caused by a blight of different insects, expressly formed for the purpose of settling upon and devouring the mucus of the intestines, and ultimately the intestines of man."

These propositions are examined in detail with a charming freedom from doubt on the part of the writer. His comments on Dr. G. Johnson's theory and practice deserve quotation:—

"The old theory proceeded upon the notion that the symptoms of cholera proceeded from an excessive drain of fluid from the blood. Dr. Johnson's view, as I collect it, is that the symptoms of cholera are caused by an impediment which takes place in the passage of blood from the right to the left side of the heart, through the lungs; and he believes that the blood in cholera contains a poison, the irritant action of which excites contraction of the minute pulmonary arteries, and thus arrests the flow of blood through the lungs.

"This appears to me to be mistaking a symptom for a cause. The sudden devouring of the mucus of the intestines by a crowd of insects is sufficient to account for every peculiarity of the cholera patient; every vital function is impeded; paralysis of the eighth pair of nerves supervenes; and though the actual quantity of blood in the system is not lessened, there is such an abatement of the energy of the heart and lungs, by which the blood is properly circulated, as to account fully for all the symptoms of the collapse in cholera" (p. 13).

This is exquisite fooling.

Cholera: a New Theory. By C. DUDLEY KINGSFORD, M.D.
J. Churchill and Sons. 1866. 8vo, pp. 16.

There is but one known poison, Dr. Kingsford, holds, which kills in as short a space of time and with symptoms like those of cholera, namely, phosphorus. Dr. Kingsford surmises that phosphorus may be the essential poison of cholera, and that it may find its way into the system in the form of phosphuretted hydrogen. He seeks to show that the conditions under which cholera is developed are those favourable to the development of the gas in question. Even in the recent outbreak of cholera in East London he finds some support of his opinions:—

"To account for the recent sudden outbreak of cholera at the east end of London, I will mention one very significant circumstance, which was told me by a highly respectable inhabitant of Whitechapel—that on the Saturday previous to this outbreak a very large importation of mackerel, packed in ice, arrived in the market, and was greedily bought up by the poor; the fish were sold for a penny a piece, and within a short time after they were exposed to the air, and consequently thawed, they were totally unfit for consumption; and that on the same evening, to his knowledge, a large quantity of meat was sold, which became putrid by the following Sunday morning. Now, in Miller's *Elements of Chemistry*, vol. i., is found:—'Sea fish in general, whiting, herring, and mackerel, in particular, soon after death exhibit a luminous appearance . . . if the fish be exposed to a cold sufficient to freeze it, the luminosity disappears, but it returns when it is thawed.'

I would ask, then, if phosphorus be the *fons et origo mali*, how can the increase of the disease in this instance be better explained?"

Even pestilences, it would appear, have their comical aspect.

A Simple Explanation of Cholera: and a Natural Mode of Treating it. By YOD, M.D. London: Renshaw. 1866. 8vo, pp. 16.

Like most simple explanations, this exacts more from the faith of the reader than more elaborate theories. In so far as the writer seeks to simplify the treatment of cholera, by urging the internal use of water, and using such medicaments only as will allay the irritation in the bowels, he is to be commended.

On Cholera: its Nature and Treatment. Being the Debate in the Harveian Medical Society of London. Edited by Dr. C. DRYSDALE, Honorary Secretary of the Society. London: Robert Hardwicke. July, 1866. 8vo, pp. 34.

A promiscuous discussion, which is not amended by the introduction of the Secretary, and which it was an error to publish.

Brief Remarks on Cholera: being the Result of Observations during the two last Outbreaks of Cholera in England; and an Attempt to advance a Theory of that Disease which shall lead to a more consistent Method of Treatment. To which is added a short Table of Practical Rules for general use during an Epidemic. By ROBERT J. SPITTA, M.D. Lond. London: J. Churchill and Sons. 1866. 8vo, pp. 15.

Dr. Spitta assumes that the cholera poison is an organism in the air, and that its mode of entrance into the system is by the food. The reception of the poison gives rise to irritation of the alimentary canal, its absorption into the system, and collapse. In the first stage opium is the chief remedy; and in the second, calomel. Neither the theory nor the practice betray novelty, and the latter does not hang very consistently to the former.

Do Small-pox and Cow-pox afford any Protection from Asiatic Cholera? with some observations. By AMBROSE BLACKLOCK, Surgeon-Major, H.M. Madras Army. London: H. K. Lewis. 1866. Fcap. 8vo, pp. 37.

The main object of this pamphlet is to sustain the proposition that persons who have had well-marked cow-pox at no distant period, say within five years, are fully protected from Asiatic cholera as well as from small-pox. "This, however," writes the author, "is not, in my mind, a positive fact, it is only an impression, but an impression strengthened to some extent by the known identity of the small-pox and cow-pox poisons, which renders it highly probable that as persons with variolous

marks have a singular immunity from cholera; those who have good vaccine scars, imprinted at a time not too remote, are also well-protected from cholera spasmodica." The author advances other arguments in support of his impressions.

Cholera Non-contagious, and the Absurdity of Quarantine Restrictions Demonstrated. By EDWIN HEARNE, M.B. Lond., F.R.C.S. Eng. Southampton: Gutch. 1866. 8vo, pp. 38

Dr. Hearne writes strenuously against the opinion that cholera is a contagious disease, and he strives to show the absurdity of quarantine against this malady and yellow fever.

APPENDIX.

*Rules for the Guidance of Sanitary Authorities, Practitioners,
and the Public during the Prevalence of Epidemic Cholera.*

By the Professors Dr. W. GRIESINGER, Dr. MAX VON
PETTENKOFER, and Dr. C. A. WUNDERLICH.

[The following instructions respecting the restraint and prevention of epidemic cholera, drawn up by three of the most distinguished German physicians, each of whom writes with special authority on the subject, and representing the most advanced teachings of the day, are so full of instruction and interest, that we have translated them without curtailment.—ED.]

Introduction.

The cholera, this year, has made its appearance early. How far it may extend is uncertain. The dryness of the year 1865, of the past winter, and of the present spring, renders doubtful any great epidemic extension of the disease in many parts of Germany. Yet we are far from knowing all the conditions of the epidemic sufficiently to have any certainty in this respect. The movements of troops in Germany, when once a few centres of the disease have been established, may cause its rapid spread in districts predisposed to it; and, even if we should escape during the present summer, who can tell when this destructive malady may again threaten us?

With regard to the spread of cholera, and the causes of its epidemic prevalence, science is in possession of certain established positions, resting upon careful examination of sufficient data. These positions are known to those medical practitioners who have closely devoted themselves to the subject; but many know them only imperfectly, or, by reason of unfounded doubts, do not hold them with full conviction. By the public these positions are almost entirely unknown; although they afford the only basis for any effectual measures of prevention, and although they are very simple and intelligible to every one. When cholera approaches a place, we commonly see among the inhabitants only a bewildered terror, and a grasping at expedients that are useless, or even hurtful, although vaunted by greed; instead of a firm resolution on the part of each one to combat the common enemy by a consistent application of the means that science and experience point out as efficacious, and by which every one can best protect his own threatened life. The sanitary authorities themselves are frequently uncertain about the utility or the necessity of the most important measures, such as quarantine, seclusion, and disinfection. It need therefore be no matter for surprise that really effectual measures are often neglected; or, on the contrary, that wholly useless or absurd procedures should be put in practice. In this spring, for instance, in one of the small epidemics, we have seen the air of the streets fumigated by burning juniper berries!

Under these circumstances it seemed to us advisable to put together, in a concise form, for the use of sanitary authorities, of the medical profession,

and of the public, those principles with regard to the spread of cholera which are firmly established on a scientific basis, and on which the chief preventive measures are themselves founded ; and to show both the scientific grounds of such measures, and the proper methods of carrying them into practice. We have done this in the following pages ; and we confidently hope that our communication will receive attention, and that the calamities which cholera epidemics entail upon human society may be diminished by the consideration and observance of what we have here laid down.

With regard to disinfection, we have furnished practitioners and authorities with a principle of practical application, which was, indeed, the basis of many of the methods formerly in use, but which had not previously been stated with sufficient clearness and precision. This principle seems to us to flow immediately from known and proved facts ; and from its complete expression we may expect a final determination of a well-defined question, and hence a step in our knowledge of, and our opposition to, cholera.

For the comprehension of cholera a right observation of the epidemics is the first necessity. We therefore thought it not superfluous to state, in the second part of our tract, the essential points to which useful observations must be directed. This part is addressed only to the medical profession and to the sanitary authorities. It states briefly the matters about which information is required by science, in order that the occurrences during an epidemic may be correctly stated, and rendered fruitful for the prevention or suppression of future visitations. It is impossible that all points of this programme of observation should be equally well carried out in all places. It is better that one part of it should be carried out, earnestly and consistently, than that all should be attempted, with insufficient means and divided force. In large or moderately large states, where chiefly there will be opportunities for a complete fulfilment of the programme, a division of labour in accordance with the different aspects of the facts is essential.

We hope that our little tract may conduce to identity of management and of observation on the part of governments and sanitary authorities.

A. MEASURES AGAINST THE EXTENSION OF CHOLERA.

It is the fact that cholera—that is, its specific cause, its germ—is disseminated through the personal intercommunication of mankind. According to observation hitherto, we may assume that this germ is contained chiefly, probably exclusively, in the intestinal evacuations of persons coming from places infected by cholera, and suffering from diarrhœa or cholera themselves. Whether persons not so suffering, but feeling perfectly well, and only coming from infected localities, are likewise capable of disseminating the germ, can, in the present state of knowledge, neither be affirmed nor denied with certainty.

Notwithstanding active intercommunication, and a presumed abundant dissemination of cholera germs, there are many times and places in which epidemics of the disease do not occur. We must therefore assume that the dissemination of the germs must coincide with the presence of certain auxiliary causes—temporary, local, or personal—in order that an epidemic may be produced. It cannot be doubted that the most important of these auxiliary causes must be due to the qualities of the soil, and to the personal condition of individuals. Hence the measures for preventing the spread of cholera must be founded upon a consideration of three essential points : 1. Upon the cholera germ in the evacuations ; 2. Upon the local peculiarities of soil, especially in the subsoil of dwelling-places ; 3. Upon the condition, that is, the state of nutrition and the manner of life of the people.

SECTION THE FIRST.

DISINFECTION.

§ 1.—*The Principle of Disinfection.*

The evacuations containing the cholera germ can be so altered by chemical agencies as to lose their injurious action.

The fresh evacuations of cholera patients, or of persons coming from places where cholera prevails, do not exert a poisonous (cholera producing) action; but differ, in this respect, from what is seen in other infectious diseases, such as small-pox, in which the sufferers communicate a mature and active infectious material to others. It is only after the occurrence of a certain decomposition and change, which very probably takes place external to the organism, that the cholera evacuations acquire the property of producing the disease in healthy persons; and it is only when the already-mentioned predisposing or auxiliary causes are in operation, that an epidemic extension of the disease can be occasioned. The cholera germ, whether we conceive it to be a poison, a ferment, or a cell, must therefore be an organic material, for the development of which certain external conditions are required.

At present no practicably applicable means is known, by which either all the organic components of urine and fæces may be instantaneously destroyed, or by which changes in them external to the organism may be prevented, so as preserve them in their fresh condition. There, is however, reason to believe that this is not necessary for the destruction of the cholera germ. It appears to be sufficient so far to modify the decomposition of the excreta, by the admixture of certain substances, that the conditions under which the cholera germ is commonly developed may be retarded. Although we are unacquainted with the exact nature of the cholera germ, and with the changes that it undergoes before it becomes active in the causation of disease, yet we are able, with great probability of being in the right, to lay hold of certain chemical indications, with respect to disinfection, in the fluids that convey the cholera germ, both before its infectious action begins and after it has acquired this peculiarity.

Every mixture of recent urine and fæces acquires, after a few days, an alkaline reaction; the result of a spontaneous decomposition that produces carbonate of ammonia. The evacuations of diarrhoea are often alkaline from the first; and the evacuations of cholera are alkaline as a rule. Experience has long since shown, and chemistry teaches, that a great influence is exerted upon the occurrence of certain changes and decompositions, in organic matter that is moist, or suspended or dissolved in water, by the reaction of the fluid portion, so that certain changes occur chiefly in acid, others in alkaline, others again in neutral fluid: and for many the one or the other reaction is even an indispensable condition.

With regard to the cholera germ, or cholera poison, it is a matter of fact that its development is in no way hindered by the presence of even a very considerable quantity of carbonate of ammonia or sulphide of ammonium (substances with an alkaline reaction); but, on the contrary, the facts show with great uniformity that the germ, once introduced, everywhere multiplies and increases the more abundantly, the more extensive and potent is the action of the constantly alkaline contents of cesspools upon the soil and the atmosphere of a house.

It must therefore be considered extremely probable that the alkaline reaction of excrementitious fluid, from the presence of carbonate of ammonia,

is among the most necessary or essential conditions for the development of the germ or poison of cholera. Upon this ground it may be anticipated that the retardation of the occurrence of alkaline reaction, or, where it has occurred already, its complete neutralization until a decided acid reaction is produced, may prevent the development of the noxious germ.

§ 2. *Enumeration of the Principal Disinfectants.*

In order to fulfil the above-mentioned disinfection several agents may be employed; and from among these we have to select such as can be procured easily in sufficient quantity, and such as exert no hurtful influence upon mankind or upon dwellings.

All metallic salts that have an acid reaction, and are soluble in water, may be used as cholera disinfectants. Among these the sulphate of iron is the cheapest, the most common, and the most easily procurable in bulk.

Chloride of manganese, a product obtained in the manufacture of chloride of lime, is of equal value with the sulphate of iron, if it be first freed from hydrochloric acid, by neutralization with metallic iron, or by other methods of removal. In the vicinity of chemical works, the chloride of manganese is commonly cheaper than an equivalent quantity of sulphate of iron; but the whole quantity produced is too small to be generally employed as a disinfectant.

A similar purpose is fulfilled by the use of the soluble salts of zinc, the sulphate and chloride. These are more expensive than sulphate of iron; but do not produce rust stains when scattered about.

The power of preserving recent excreta in an acid condition is possessed also by many other substances; among which carbolic acid (hydrate of phenyl, Frankfort creosote) demands prominent mention. It can be made from coal in great quantity; and, since, for this purpose, purity is not required, at a cheap rate. Unfortunately, it is not now to be procured in sufficient bulk for general use as a disinfectant; and, moreover, it is useless without the simultaneous application of a metallic salt (sulphate of iron), in cases where it is required to restore the acidity of excreta that have already become alkaline. The preserving power of the metallic salts may, however, be very greatly increased by an extremely small addition of carbolic acid. A solution of carbolic acid may be considered equivalent to crude pyroligneous acid.

The substances already mentioned are all employed in a liquid form, dissolved in water; but there are cases in which a gaseous disinfectant is required, as for example, when places are to be disinfected (such as irregular cesspools, inaccessible drains and sewers) which offer insurmountable obstacles to a complete saturation with fluid. In such cases we should employ volatile or gaseous acids, among which the sulphurous acid is chiefly to be recommended. It may be obtained by the combustion of sulphur or of sulphur matches, or by treating sulphites with concentrated sulphuric or hydrochloric acid.

The agents already enumerated act in accordance with their chemical qualities, by retarding or preventing the alkalinity of the excreta. Besides them there is yet another substance that has been largely used as a disinfectant—namely, chloride of lime. There are no certain facts known with regard to its action; and, although it would perhaps be erroneous to pronounce it wholly inoperative, it would certainly not be wise to place, in the same category with the preceding acid reagents, a substance of an entirely different nature, that by its alkaline reaction could only interfere with their efficiency, and that moreover cannot be obtained in quantities, and at a price to render it available as a general disinfectant.

§ 3. *The quantities in which Disinfectants should be employed.*

The question as to the quantity of any disinfectant that should be employed may be answered by saying that the end sought is attained when the excreta, and all that is mixed with them, possess a decided acid reaction; and retain it until they can be moved away from the vicinity of human dwellings.

We may assume that twenty-five grammes of sulphate of iron (or an equivalent quantity of the salts of zinc or manganese) dissolved in water, would, on an average, be a sufficient daily quantity for each person. This calculation supposes that the population is made up of persons of all ages in the ordinary proportions; and that the recent excreta are not added to old collections, already in a state of alkaline decomposition. Such collections should either be entirely removed at the beginning of the disinfection; or, what is more simple, should be so liberally treated with the acid agent, as to completely destroy their alkalinity.

The quantity of twenty-five grammes is taken as an average for adults and children, for the diseased and for the healthy. A mixture of recent urine and fæces from a healthy person is almost always acid; but a similar mixture, from a patient suffering from diarrhœa, is very often alkaline when voided.

When such a mixture of excreta is actually acid, it can be kept so by a very small addition of carbolic acid. Where there is the opportunity of employing this agent, it is greatly to be recommended; since it not only perfectly fulfils the purpose of a disinfectant, but also represses more than anything else the fœtor of the excretions. Three grammes of pure carbolic acid, or four grammes of an acid not perfectly pure, as it is first separated from the crude carbolate of soda, dissolved in 100 grammes of water by agitation, will suffice for each person daily; supposing that the excreta are already acid.

§ 4. *The objects to be disinfected.*

The disinfection must have reference first to the excreta, and then to all contrivances or apparatus for holding, collecting, or conveying them; and generally to everything in which excrement has been contained. The excreta, whether urine, fæces, or matters vomited, should be discharged by the patient, when possible, into vessels already containing the disinfectant. Not only the evacuations themselves, and all utensils, buckets, water-closets, cesspools, sewers and pipes that may have contained them should be disinfected; but also soiled linen, clothing, or wooden floors on which excreta may have been spilt. The intestinal contents of cholera corpses, and everything soiled by them, must be treated in the same manner.

The medical advisers of the different local authorities should make such suggestions, that the rules above laid down may be adapted to the special conditions of each place.

For the disinfection of soiled linen and clothing, and also of wooden floors, the chloride of lime has heretofore been generally employed. With respect to it we can only refer to what has been said already. Sulphate of iron, and chloride of manganese containing iron, would injure clothing and floors, by covering them with rust stains. Solutions of carbolic acid in water, or of the salts of zinc, have not this disadvantage. The carbolic acid produces great annoyance by its very persistent odour, and when applied to floors renders them extremely unpleasant for a long time; so that for linen and such matters watery solutions of sulphurous acid, or of sulphate or chloride of zinc are to be preferred.

Above all things the public should be apprized of the important truth that the universal experience of practitioners and nurses proves that the recent evacuations, even in the most acute form of Asiatic cholera, are not sources of danger ; and that there is the less to be feared the more speedily the proper steps for disinfection and cleanliness are taken.

It is self-evident that the most complete possible removal of all organic remains and foul substances from the vicinity of human dwellings, and the destruction of all worthless or suspicious refuse should be strictly enforced but never without a preceding thorough disinfection.

§ 5. *When the Disinfection should be commenced.*

It is an important question where and when disinfection should be commenced. In every epidemic of cholera it has been observed that many places, notwithstanding constant communication with other places smitten by the disease, have remained free, or at least have suffered no epidemic visitation ; and also that places attacked in certain years have escaped in others, although no change has occurred in the intercourse or manner of life of their inhabitants. As reasons for these important phenomena it has hitherto only been suggested that peculiarities of soil may act as local, and variations of the earth's moisture as temporary causes of exemption. Upon this question the most important points will be referred to in the second section.

The determination of the question what places, or parts of places, or neighbourhoods, and what periods of time, are most favourable to the development of a cholera epidemic, must depend upon careful local observation and research, such as hitherto could only be carried out long and closely enough in the smallest districts.

When the introduction of the disease and its epidemic development in any place is to be feared, we ought not to wait with our disinfectants until the epidemic character of the outbreak has been shown in several houses and cases. The disinfectant should not, as has often formerly been the case, follow the steps of the cholera from house to house, but should precede it. Disinfection is only important as a prophylactic.

When the disease has been introduced into a house, and an indubitable case of cholera has occurred among the inmates, it will, as a rule, be too late to disinfect ; and when the patient has been infected in the house itself, the opportunity of receiving the poison will usually have been afforded at the same time to all the other inmates ; and it will depend essentially upon the condition of individuals whether or not the disease will be farther developed. Notwithstanding this, the use of disinfectants should never be omitted in houses where cholera has appeared, since they will at least prevent the further development of the germs.

When a case of cholera has appeared in a single house in a place, there is the more reason to hasten to disinfect the other houses, since the germs from the first may already have been conveyed to them, even before the nature of the disease has been medically and officially certified.

The concealment or neglect of the first case of cholera in a place is one of the greatest errors that can be committed, and usually occasions more injury than can be afterwards retrieved by the greatest efforts and sacrifices.

The water-closets of railway stations and hotels must be constantly disinfected so long as the introduction of cholera by travellers is to be feared.

The foul linen of strangers in hotels must be disinfected before it is sent to a laundress.

The period at which disinfection may be abandoned depends essentially

upon whether the possibility of the introduction of germs, or the period of local predisposition to the disease, has ceased. In order to determine these points with sufficient accuracy to obtain a sure basis for practice, further investigations are required.

§ 6. *Superintendence of disinfection.*

The actual carrying out of the disinfection may be left to the owners of the separate houses, although it is better undertaken by the local authorities ; but in either case it requires careful medical supervision. This supervision must determine that no alkaline reaction shall occur in any place where excreta are collected or conveyed ; and that, if such should occur, it should immediately be supersaturated by acid.

In order to show the acid reaction it is sufficient to place a drop of the fluid, by means of a glass rod, upon a slip of litmus-paper, and to observe that this is reddened.

When the reaction is alkaline, this may be shown by placing a drop of the fluid in the same manner upon yellow turmeric paper, which will be turned to a red brown.

If it is desired to test the air of drains, sewers, or pipes, for the presence of carbonate of ammonia, a slip of turmeric paper must be moistened with distilled water, and placed for half its length between two slips of glass. The whole must then be placed for a few minutes in the suspected air. The presence of the smallest quantity of ammonia will produce a marked difference in colour between the covered and the uncovered portions of the paper.

§ 7. *Limitation of intercourse.*

Since it is not to be doubted that the spread of cholera depends upon the intercommunication of mankind, it may be assumed that the spread would cease if all communication were suspended. But, as a complete suspension of intercourse would be a greater calamity than cholera itself, so have all ordinances tending in such a direction proved hitherto fruitless and illusory. Our efforts must be limited to an endeavour to render intercourse harmless, by strict enforcement of disinfection.

If the present opinions about the conveyance of germs, and about the essential nature of disinfection, be correct, it follows that the latter may afford as complete a protection as absolute arrest of intercourse, or as the natural immunity of certain places.

It is only on the sea coast and in seaports that an arrest of intercourse can be enforced with good results ; when ships coming from infected ports are prevented from landing anything until after the lapse of the longest period of incubation that has been observed in cholera ; or when the crew and passengers are kept in strict quarantine for the same period of time.

Such quarantine should be maintained for at least four weeks, and should be so arranged that the arrivals can communicate no infection to persons departing.

The disinfecting regulations must be most carefully observed in all quarantine establishments.

SECTION THE SECOND.

ON THE LOCAL OR TEMPORARY PREDISPOSITION.

The local or temporary predisposition is chiefly influenced, according to the present state of the inquiry, by the permeability of the soil by water

and air, by its varying fluid contents, and by its being impregnated with organic and decaying nitrogenous materials.

A soil that is impermeable, or but slightly permeable, by air and water (a close rocky soil, for example), is little or not at all liable to an epidemic outbreak.

Porous soils, and even rocky soils that are split up by numerous and deep fissures, filled in with earth, do not afford the same protection.

When an impregnated porous soil has been unusually saturated with moisture, so that the air has been forced out of it for an unusual time and to an unusual height by water, the rapid subsidence of the water favours the epidemic development of cholera in such places.

The more the surface layers are impregnated with decaying organic matter, the more dangerous will be the recession of the surface water, in case the germ of cholera should be introduced at the time.

The recession of surface water, and the consequent drying of soil that has been thoroughly soaked for some time, appear to be of the greatest weight with regard to the time of outbreak of a cholera epidemic.

In river channels, in valleys, and at the feet of steep declivities, the above three factors are often in combined action ; since these conditions of surface promote the formation, collection, stagnation, and variation of surface water.

Localities upon a ridge between two valleys, or between two water-sheds, show generally a much less degree of predisposition.

The courses of rivers very constantly show a less predisposition the nearer they approach to their water-sheds.

Against peculiarities of soil, surface water, and poisonous impregnation, scarcely anything can be attempted at short notice. When the introduction of cholera germs coincides with the presence of these three factors in an unfavourable sense there is nothing to be done, save disinfection, but to avoid or desert the locality.

The above considerations are highly important, not only for those who fly from cholera, but also as guides in the choice of places for cholera hospitals or quarantine stations, and of camping grounds for soldiers, railway makers, or other workmen. Although it may often happen, in war, that strategic considerations have little choice of place, yet still this choice should be exercised as far as the demands of strategy will permit. A judicious preference of high levels with compact subsoil is all the more important, when it is impossible to insure perfect disinfection of all excreta.

SECTION THE THIRD.

UPON INDIVIDUAL PREDISPOSITION.

In every house or place attacked by cholera, the greater number of the inmates are equally exposed to the epidemic influences of the germs and of the soil ; and most of them experience at the time of an epidemic some change in their condition, although it is only in a comparatively small number that this change amounts to a dangerous outbreak of disease. The power of resistance against the epidemic is very different in different people.

In so far as the transudation of water from various organs into the intestinal canal is the most essential phenomenon of cholera, everything is of importance to the individual by which such transudation is promoted, favoured, or occasioned. Among such influences are all by which the bowels are overmuch irritated or relaxed, all which drive the circulation from the surface of the body to internal organs, and all which either increase the

normal fluid contents of the organs, or retard the normal discharge of water from the body.

Every person, therefore, should carefully avoid all influences which his experience tells him are likely to produce diarrhœa in his own case; and, if attacked by diarrhœa, should immediately seek medical aid. Medical house to house visitation of the healthy, so as to detect all illness at its commencement, has, in all epidemics, been of the greatest benefit to the poorer classes.

The establishment of stations for the care and observations of persons suffering only from diarrhœa, besides the special cholera hospitals, is greatly to be recommended. For such stations healthily-placed localities should be selected.

A natural state of constitution being pre-supposed, a great influence is exerted on the general condition of the body by food, drink, clothing, residence, and occupation.

The consumption of tainted provisions and of impure water is, of course, to be avoided. The diet should be moderate, but supporting. A suitable blending of well-cooked soup, meat, and bread, in quantities proportioned to the digestive power, with light puddings of eggs and flour, and with vegetables, is to be recommended.

A large consumption of any kind of fluid should be avoided; and only so much taken, either of water, wine, or beer, as may be needed to satisfy thirst. Persons who are habitual spirit-drinkers in any quantity furnish numerous victims to the disease. The drinking water should be pure and bright, the alcoholic drinks genuine and well fermented.

A sudden change of diet produces no immediate corresponding improvement in the state of the bodily organs; and it is often some weeks before the general condition is raised to the level of a better diet. At times when cholera is approaching or has appeared, the whole population should be better nourished than usual.

The clothing should afford a sufficient protection from cold, without checking transpiration. Being chilled will often drive the circulation from the surface of the body, and occasion congestion of internal organs or catarrh of the mucous membranes. The abdomen especially should be warmly clothed, which may be suitably done by a flannel bandage. Good beds and clean linen are important aids to uninterrupted transpiration.

Promotion of the functions of the skin by internal means, such as warm drinks (peppermint tea, chamomile tea, warm wine, and the like), is a matter that should be left to the medical judgment in each individual case. The same rule applies to the use of vapour, Roman, or Turkish baths.

The dwelling has the greatest influence upon the air that we breathe, that constantly surrounds us, and that uninterruptedly must yield us oxygen, and must withdraw from us proportionate amounts of heat, water, and carbonic acid, in order to preserve the normal condition of our bodies. Long continuance in a confined atmosphere, which withdraws too little water and carbonic acid, is shown by experience to increase the disposition to cholera in a high degree. The absence of fresh air, bad ventilation between the decks of overcrowded ships, in crowded barracks, prisons, or rooms that are too small for the number of inhabitants, has been shown by much experience to be a frequent cause of violent choleraic outbreaks. Among persons who have received the cholera germs in some infected place, and have afterwards been compelled to live in too little (that is, in much vitiated) air, the individual predisposition will be so much increased in a few days that many will be attacked by the fully developed disease; while others, infected at the same place, but afterwards living in a better air, often suffer very little, or even not at all.

During a cholera epidemic, therefore, all dwellings should be well and uninterruptedly ventilated, and kept thoroughly clean. The perils which are frequently and erroneously ascribed to too great a current of air, to what is called a "draught," may be obviated much better by clothing, bedding, heating, and so forth, than by shutting up the doors and windows.

No one can believe that the inclosed air of a house is better than the air of the street; but the house cannot generate its air for itself, and must obtain it from the street, generally in its immediate vicinity.

In a foul and stinking atmosphere the pernicious elements cannot be destroyed by an admixture of strong smelling matter (fumigation); but, as a rule, the objectionable smell is only concealed by another that is stronger, although more bearable. The air can only be really improved by ventilation, which dilutes all foreign matter contained in it.

The smaller or more crowded any house or chamber, the more necessary is complete ventilation.

It is a practice sanctioned by long custom to place chloride of lime in rooms containing a tainted atmosphere; although there is no proof of the smallest benefit from doing so. Chlorine certainly produces changes in most organic substances; but, if it were introduced in sufficient quantity for the disinfection of a dwelling-room, it would render the air of the room no longer respirable. We ought not to forget, moreover, that the human body is itself an organic substance, which the chlorine may attack.

If it be desired, during a cholera epidemic, besides a sufficient ventilation, to diffuse some odour through a dwelling-room or sick chamber, the purpose is best fulfilled by some volatile acid, together with some æthereal oils. The acid should not affect the respiration. The sprinkling or evaporation of vinegar, or of acetic acid, in such quantity as to fill the air with the odour, can never be hurtful; and the acetic acid, on the principle already laid down, may be supposed to exert some power as a disinfectant.

Occupation and bodily movement, in a certain degree, are not only conducive to health, but positively essential to its preservation. They must not be carried too far, nor allowed to produce great fatigue or exhaustion. Excessive exertion has a predisposing influence to disease, like debauchery, or excesses of any other kind, over eating or drinking, acute emotions, and so forth.

Where a daily regular occupation is necessarily followed in a room, daily exercise should also be taken for some time in the open air. On days when the weather prevents going out, the exercise may be taken in a room with open windows.

SECTION THE FOURTH.

REGULATIONS FOR ARMIES IN THE FIELD.

Even for armies in the field it may often be very possible to guard against cholera, and to check its extension and its dangers, both for the troops themselves, and for the population of the seat of war. The claims of war will, in very many cases, not preclude due carrying out of prophylactic measures; and, even for military results, such measures will often prove of greater advantage than successful battles.

1. It is self-evident that places in which cholera prevails should generally be avoided by troops on the march. It is true that to march through such a place, without halting, may be considered free from danger; but any halt, even for a few hours, either of detachments or of individuals, may bring cholera into the army—to break out mostly soon, but possibly only in from two to four weeks after its introduction. It is under all circumstances advisable

for troops to encamp in the neighbouring open country, rather than to go into quarters in a town infected by cholera. In large towns it may often happen that cholera is epidemic in certain parts, while others remain free on account of local advantages. When the military occupation of such a town appears to be necessary, the troops should take possession of the healthy parts only; and all traffic with the infected districts should be strictly prohibited. When a division is joined by recruits or reinforcements that come from places where cholera exists (although they do not bring any diseased persons with them), it is prudent to quarter the new comers in a detached position for at least fourteen days, there to undergo careful medical observation and disinfection.

2. Where the possibility of choice exists, we should select the highest possible places and the dryest and hardest ground, such as the ridge of a water-shed, for the encampment of troops, and never excavated or moist ground. All excrements should be disinfected as a prophylactic measure.

3. If any cases of cholera, or suspicious forms of diarrhœa show themselves, then—

a. All cholera cases should be immediately separated, and placed in a special hospital at some little distance, or still better, in tents or huts. These should be set up at one side of the position of the troops, and on the dryest and most compact soil that is accessible; and the evacuations and clothing of the sick are to be treated in the way already described.

b. The cases of diarrhœa should also, when circumstances allow, be kept separate, and brought to special stations for observation and treatment, to prevent the outbreak of cholera: these evacuations being constantly disinfected by sulphate of iron. Where the circumstances do not allow this, the men suffering from diarrhœa should at least be relieved from arduous duty, should receive an improved diet, and should be made to wear an abdominal bandage, and to take proper medicines, especially small doses of opium. It must be made a point of duty for every man attacked with diarrhœa to report himself immediately to the surgeon; and daily medical inspection should be made with regard to fresh cases, and of the state of those already under treatment.

c. When cholera threatens an army, each division must have a diet regulated in the manner above laid down. The men must be cautioned against drinking much water, or much drink of any kind, against sour provisions, unripe fruit, and the like, and must take a comparatively dry flesh diet, with coffee, and a little brandy.

d. All fatigue and exertion of the troops that is not imperatively required should be forbidden during cholera time; such exhaustion certainly increases the liability to the disease.

e. The existence of cholera in an army should never be concealed; and if a division suffering from it is coming into a town previously free, the presence of the disease should be made known at once, and even before the arrival of the men, in order that proper disinfectant and prophylactic measures should immediately be commenced.

4. Any division of an army that has already suffered from cholera obtains thereby, for a long time, a diminished predisposition, or even immunity. If, therefore, it is necessary to occupy or to reconnoitre a neighbourhood that is infected, and troops so seasoned are to be had, they should be selected in preference to all others.

B. SCHEME FOR THE OBSERVATION OF CHOLERA EPIDEMICS.

§ 1.

In the first place, the manner of occurrence of the first case of cholera in any place must be inquired into.

The chief questions are :—

Had the person first attacked visited within four weeks any place in which cholera prevailed ?

Are there, in the house in which the first case occurred, strangers arrived from any place where there is cholera ! If so, are they, α , cholera patients, β , diarrhoea cases, γ healthy persons, or, δ , corpses dead from cholera ?

Have any effects from a cholera-place, especially the soiled linen of cholera patients, been brought into the house ?

Has the person first attacked visited (if he has not inhabited) houses into which cholera germs may have been introduced in any of the ways mentioned above ?

What description of individual was the first attacked ?

Has he been exposed to any powerful occasional cause ?

What sort of care did he receive ?

The time and place of the commencement of the first attack should be noted with great care.

§ 2.

With regard to the observation of the spread of the epidemic in a place, the first thing is to collect, from first to last, a daily list of occurring cases and of deaths ; with mention of the house, story, age, sex, and condition (for this we append a simple table for the cases of death). The deaths should be published daily, with the streets and the numbers of the houses ; but the occurring cases should not be published.

The inquiry should be conducted as far as practicable into the occurrence of cases in which the infection has been conveyed by individuals or fomites ; and any clear and undoubted example of the conveyance of the disease, in which the influence of soil and place of residence could be satisfactorily excluded, should be scrutinized with the closest observation.

The possible action of infectious matter in a recent or already dry and changed condition, as in soiled linen and clothing, is to be noticed.

Any clear and certain facts about the time of incubation of the disease should be recorded.

Positive and negative testimony should be collected with regard to the spread of the disease to neighbouring places, and with regard to the means of its extension. Also with regard to its extension along lines of railway.

Special investigation is required where the disease has been epidemic in autumn ; and, after a pause during winter, has broken out afresh in the same place in the spring.

§ 3.

Concerning the auxiliary causes of an epidemic, the attention should first be directed to the geological character of the soil in the locality generally, and to the position and peculiarities of the substrata of the houses most severely visited (after the close of the epidemic, to those also that were least visited), whether they be rock, loose stone, detritus, sand, or loam. The strata of the locality should be observed from the surface to the bottom of

the water springs. Where different strata overlie one another, their average height should be given : and it should be noticed whether one or other of them may occasion collections of surface-water from time to time.

Attention should next be directed to the level of the surface-water. If no former observations have been made upon this point, it is always of interest to make them during the epidemic, and to examine also the height of the neighbouring springs. Where these do not flow over or from the first impermeable or water-guiding stratum, and where, therefore, the height of the springs is not a standard for that of the surface-water in the vicinity, special shafts should be made, in order to compare the levels at the close of the epidemic with those of a later period. Information should also be sought from the owners of springs, and from other trustworthy persons, with regard to the water-levels and the moisture of the soil at the period immediately preceding the epidemic.

The houses that at the close of the epidemic have been most heavily visited must be the subjects of special scrutiny, mostly of an obvious kind. Their high or low position, the stratum on which they stand, the position of neighbouring sunken ground, the vicinity of running or stagnant water, or of heaps of pestilential matter, the building materials of which they are constructed, the degree of moisture of the houses themselves, and of their court-yards, the condition of their closets, sewers, and traps, and the effluvia from them, the number of inhabitants in each house, the state of nutrition and general health of the inmates, and the state of their sleeping-rooms, dwelling-rooms, and workshops, comprise the chief points that should be noticed.

§ 4.

An actual extension of cholera has sometimes appeared to be due to drinking water ; and in other cases the first impression to this effect has been refuted by accurate examination. Inquiry should be made into the source from which the inmates of the severely-visited houses obtained their drinking-water, whether from the same source as many persons who remained unaffected. The peculiarities of any water suspected of propagating cholera should be noted, especially whether it throws down any dirty deposit, or whether it is believed or can be proved to be contaminated by cholera excrement.

§ 5.

The constitution of the individuals attacked should be inquired into ; with particular reference to any changes shortly before the outbreak of the disease. Any abuse of alcoholic drinks should be noted. Fear, colds, dietetic errors (their nature to be specified, and whether they loaded the organs with too much water, or affected the intestinal mucous membrane), misuse of medicines (what medicines), are all points to be observed.

New and interesting observations may be made with regard to the action of the epidemic influence upon the healthy during the prevalence of cholera, —that is, upon persons neither attacked by cholera nor by diarrhœa. Did they experience any scantiness of urine, any tendency to cramps, &c. &c. ? —and how far were such symptoms due to changes in their diet and manner of life ?

§ 6.

Meteorological observations during the epidemic are of no value, except when compared with others previously kept over a long period or made at other places.

On the International Sanitary Conference, and the Preservation of Europe from Cholera.

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(*A Paper read before the Epidemiological Society, December 3, 1866.*)

[Elsewhere we have briefly commented on the Reports of the International Sanitary Conference. The results of the Conference form so important a contribution to State Medicine, that we subjoin the following summary, by one of the British Medical Commissioners.—ED.]

“MR. PRESIDENT AND GENTLEMEN,—At the request of your Secretary, Mr. Radcliffe, I have drawn up for submission to the Society a sketch of the principal recommendations of the International Sanitary Conference of Constantinople for preventing the diffusion of epidemic cholera, and for the preservation of Europe from future invasions of the disease.

“The Conference has concluded that epidemic cholera originates entirely in India, and never in Europe ; that it is a transmissible disease, following in the wake of man, and not carried by the atmosphere to long distances ; that it is spread chiefly by the evacuations of choleraic patients ; and that it rages most in localities which are in bad sanitary conditions. It believes that the cholera-poison may adhere to the surface of clothes, walls of houses, insides of ships, &c. ; and that these may be the means of communicating the disease to persons coming within their reach.

“Starting from these principles—the grounds of which it would be impossible to enumerate in the compass of this paper—it has based its measures of preservation ; and asserts that Asiatic cholera is to be met by measures of restriction of intercourse when practicable, by measures of purification and disinfection, and by measures of hygiene.

“In the application of these measures the Conference has held in view the following objects—viz., to prevent the development and the spread of cholera in its place of origin—in India itself, and its exportation therefrom ; and, in order to preserve Europe, to check its advance westward from India ; and to limit its diffusion in Europe should fresh invasions occur.

“The Conference has not always gone into very minute details ; but has rather contented itself with indicating in a broad manner the nature of the measures to be taken, leaving much of the minutiae to be settled by the local knowledge of those who will apply them.

“Inverting the order of enumeration above mentioned, I will speak of the recommendations in the following order :—

“1. Of the sanitary measures.

“2. Of the measures of purification.

“3. Of the measures of restriction.

“Sanitary measures.—The Conference recognises in the fullest manner the absolute importance of attention to the sanitary conditions of localities as a means of checking the development of cholera epidemics; and adopts the principle that against these safety for populations is to be found in the purity of their air, in the goodness of their drinking-waters, and in the cleanliness of the soil on which they live. In accordance with these views, it points out the necessity of sufficient space for dwellings, of ample room for lodging, and of free ventilation with air that shall be pure. To obtain the latter, it shows that, space and ventilation being secured, the purity of the atmosphere depends upon the cleanliness of the surface of the soil, and the non-impregnation of its substance with organic decomposing, and especially excrementitious matters. It believes that it is vain to expect that a town shall possess a pure atmosphere, one in which the choleraic poison has little tendency to multiply, if the soil is loaded with these matters. Hence the greatest care should be taken to prevent its infiltration with such. For this it asks for the abolition of all privies, with cesspools or wells, and the substitution for them of moveable receptacles, such as earth-closets or ‘fosses mobiles,’ which can be frequently emptied and cleaned, and which may be so arranged that fæcal matter cannot soak into the soil. The contents should be carried out of the towns. The Conference objects to the system of drains or sewers in communication with houses, holding that in practice we cannot prevent the diffusion of noxious gases through the houses in connexion with them, and that in times of cholera drains may spread the disease along a line of houses in communication with them; and that, owing to the porosity and rapid decay of masonry work, they readily allow of the impregnation and saturation of the ground through which they run with decomposing organic matters. When used, sewers should never be allowed to empty themselves into rivers, and waterclosets should never be within the houses themselves. Burial-grounds should not be permitted within towns. Slaughter-houses and noxious trades should be forbidden. Not only should it be an object to prevent all human organic or excrementitious matter from penetrating into the soil of towns, but every sort of organic refuse should be speedily removed, before the atmosphere can become contaminated by their decay above or under the ground. In short, for a healthy town, no decomposing organic matter should penetrate its soil or rest upon its surface. As the decomposition of organic matter is retarded in a dry soil, towns should always be provided with surface, and where necessary with subsoil, drainage. As seaports are frequently the first places to receive arrivals from infected regions, it is obvious that it is of the highest importance to place them in healthy states, even more than it is for inland towns. The absolute necessity for pure water, and the great danger of the use of water contaminated by choleraic matters, are pointed out. The liability of contamination of the water of wells or streams near and among which cesspools or sewers lie is shown, as well as its unfitness for use under any circumstances, and especially during cholera epidemics. It proposes filtration or boiling for water containing ordinary impurities, where none other can be had.

“The sanitary conditions insisted on by the Conference demand for their perfection the exercise of public and private hygiene. It may be doubtful how far in the present state of society private hygiene will assist in the great work; but all that may be called public hygiene in these recommendations may certainly be carried out, to the great advantage of the populations concerned. Public hygiene can regulate the width of streets, the space for habitations, the space for individuals, the direction and the termination of sewers and drains, the description of privies, the scavenging of towns, and the provision of pure water. All these are the prime agents in combating

the conditions in which cholera epidemics flourish ; and if they could be faithfully carried out, as they might certainly be, the shortcomings of private hygiene would be of comparatively little moment. The Conference has not ignored the difficulties of obtaining the desired state ; but it looks forward to the time when, to quote its own words, ‘the minds of all men shall be penetrated with the truth that most endemic and epidemic diseases owe their violence and their spread to the massing together of people, and to the fatal customs prevalent among them. Then all will understand that it is in the power of man, at the same time that it is his duty, to overcome by his efforts that condition which he has created by his ignorance.’ These measures are not to be taken only when cholera epidemics are near, but should be acted on at all times and in all seasons. They should be made permanent conditions. They may be costly, and they are in opposition to the system of sewerage in practice in England. It is probable, however, that reflection will show, and time will prove, the much greater security and efficiency of systems which seek to maintain the soil free from dangerous matters over those which allow them to permeate its substance. I will conclude this brief outline by saying that the Conference believes that if man is to be preserved from cholera epidemics, he must live on a clean soil, drink pure water, and breathe a sufficiency of pure air, and that under these conditions cholera will rarely rage around him.

“The sanitary recommendations are not confined to towns, but extend to ships, for which the importance of the limitation of the number of passengers, free ventilation, and provision of pure water free from all taint of choleraic discharges, are fully insisted on in the recommendations on naval hygiene and its appendix.

“*Measures of purification.*—To destroy the poison adherent to surfaces, and to prevent these from becoming sources of danger, disinfection and purification of houses, ships, clothing, &c., are required, and recommended upon the same principle as they are for other contagious diseases ; but, in the opinion of the Conference, cholera requires in addition the chemical disinfection and destruction of all cholera evacuations, both of confirmed cholera and of diarrhoea. These should always be disinfected separately from common excreta, and never mingled with the contents of common privies or drains. They should be buried deeply out of harm’s way, and where they cannot infect drinking-water. The Conference recommends that a general disinfection of drains, sewers, privies, and cesspools should be carried out by authority throughout the duration of a cholera epidemic, and until its extinction ; that this disinfection should begin in a town immediately that the cases of diarrhoea in the track of an epidemic show the least tendency to increase ; and that the use of all common privies should be forbidden to diarrhoeal patients during the prevalence of cholera. All linen soiled by cholera evacuations, or which has been in use by cholera patients, should be plunged immediately into solutions containing chloride of zinc or lime, or chlorate of soda, and remain immersed for twenty-four hours before washing, which ought to be thoroughly done, and combined with boiling of such articles ; and that all articles which cannot be so treated, such as bedding and thick materials, should be burnt. The temporary abandonment of infected houses, barracks, and dwellings of all kinds, and their purification by free ventilation, sprinkling or washing the walls with solutions of chloride of lime or carbolic acid, and by the diffusion of sulphurous or nitrous acid gas, or chlorine gas, and whitewashing, should invariably be carried out ; and several days, at least eight, should be allowed to elapse before their reoccupation. Measures of similar character are recommended for contaminated ships.

“The purifying agents most relied on are chloride of zinc, sulphate of iron, carbolic acid, sulphurous acid and nitrous acid gases, free ventilation, and a temperature of 212° to 250° Fahrenheit, for articles that can be submitted to it without damage. Certain kinds of goods, such as old stuffs, rags, skins, &c, should also be disinfected. It is not proposed to submit general merchandize to disinfection. The bales of goods, however, have to be landed when a ship has to be disinfected.

“The opinion of the Conference on measures of disinfection and purification is expressed in the following resolution:—‘That measures of disinfection applied to cholera on a well-arranged plan, and with perseverance, offer themselves as powerful auxiliaries—1st, for diminishing the liability of a locality threatened with cholera; 2nd, for destroying the germ of the disease; and 3rd, for limiting, in certain favourable circumstances, the spread of the epidemic.

“*Measures of restriction.*—A belief in the propagation of cholera by human intercourse naturally leads to recommendations of measures for limiting such intercourse, and these have been proposed by the Conference. It considers restriction as regards *cordons sanitaires*, isolation of the sick, complete interruption of communication between infected and healthy places, and the modified interruption known as quarantine. Restriction of intercourse has to be considered as regards land and sea, and, as applied to ships and passengers, demands the machinery of bills of health, inspection previous to embarkation at infected ports, lazarettos, and means of purification of contaminated or suspected objects.

“With regard to sanitary cordons, the Conference concludes that these, established in the midst of numerous and dense populations, are uncertain in effect; but that, on the other hand, employed over limited districts, or in countries in which the population is thinly spread, as in certain countries in Asia, cordons are of great use against the extension of the disease.

“With regard to isolation, it believes that when it can be applied to the first cases which mark the beginning of an epidemic it is a measure of prudence which no country desirous of its own safety should neglect.

“With regard to total interruption of communication, it recognises this as efficacious if practicable, but admits the impossibility of carrying it out in general; and contents itself with stating that interruption of communication is the best method of isolating cholera centres, and that in consequence it should be applied in all cases in which circumstances permit of its efficient performance, but that this measure—only applicable over limited districts—becomes impracticable and inefficacious when an epidemic is spread over a large space.

“On the question of checking emigration from infected towns to surrounding places, the Conference recognises the great evil of the flight of masses of people from the seats of epidemics, and its influence in spreading the disease. It thinks that, without preventing persons from moving from infected towns, it would be well if they could be restricted to limited areas around these diseased localities. With regard to people moving away by sea, it would limit the number of passengers in each ship, and submit them to medical inspection and their goods to disinfection before embarkation.

“Quarantine is to be considered in its land and sea aspects. Land quarantines, from what has been already remarked, have not been often applicable; but in cases of caravans, troops, and masses of emigrants, it may be found occasionally very valuable. Maritime quarantine, however, on account of the greater facility of maintaining it, is considered to be much more likely to be efficient than that on land, and the conclusion was adopted that these quarantines, established on a rational basis and in conformity

with the progress of science, may serve as effectual barriers to the invasions of cholera.

"The quarantines recommended are of two kinds—quarantine of observation and strict quarantine.

"1. Quarantine of observation consists in keeping separately and under surveillance a ship, its passengers and crew, for a period of some days from the time of the admission on board of the health guardians, the time to be regulated by the local sanitary authorities. It does not require the disembarkation of the passengers, nor the discharge of goods or merchandize, unless they be injured or in decomposition. It enjoins free ventilation of the ship and general measures of hygiene, but not its disinfection. It may be gone through at any port in which a sanitary establishment exists. In some special cases the passengers may be disembarked and landed at the lazaretto. It is applied to ships in good sanitary conditions, which have not suffered from cholera.

"2. Strict quarantine is the isolation for a fixed time of the ship and persons, with disinfection of all that may contain the seeds of the disease. It demands the discharge of all the merchandize into the disinfecting houses, the disinfection of certain articles or goods, and the landing of passengers at a lazaretto. It is applied, 1, to ships from an infected port with a foul bill of health, with certain exceptions; 2, to ships which have had cases of cholera on board during the voyage, although they may have a clean bill of health. Strict quarantine begins with ships in ballast when the quarantine officers commence their watch on board; for other ships, after landing of the goods; for persons, immediately that they enter the lazaretto.

"Thus, according to the Conference, quarantine of observation is a term of probation, of simple watching; while strict quarantine consists in the landing of passengers at a lazaretto, with the use of disinfection and of every measure of precaution that can be applied to arrivals from infected ports.

Time of separation of suspected persons.—The Conference has recommended by a majority the period of ten days as the time of separation or isolation of arrivals from infected places both for land and sea quarantines. This term has been fixed upon because, while it seems likely to accomplish all that we expected from quarantine, it will not be so onerous to the interests of commerce as the periods of sequestration adopted by some nations during the present epidemic. The Conference in this matter has endeavoured to regard both the demands of commerce and the interests of science.

"No part of its task has been more difficult than the fixing of the term of probation. The period of incubation of cholera has not been anywhere determined with accuracy. On the one hand, it may be very short, probably less than one or two days from the reception of infection, as in cases of confirmed cholera occurring rapidly after exposure. On the other hand, we cannot tell exactly how long a time may pass after exposure before symptoms of diarrhoea begin to appear, or how long a diarrhoea may exist or be concealed before it passes into unmistakable and characteristic symptoms of choleraic disease. No quarantines can be efficient unless the element of diarrhoea is taken into account, as it is probable that persons with this affection are very active agents in propagating the disease, and more dangerous than others to the community, because they wander about unsuspected for some days after the commencement of their symptoms. Still it appeared to the majority of the Conference that in the larger number of cases a true choleraic diarrhoea would in the course of eight days have pronounced itself sufficiently to have prevented concealment of its existence.

It was thought, therefore, that in fixing ten days as a period of sequestration, sufficient time would have passed to permit of the discovery of disease. Were the existence of all cases of diarrhœa discoverable, the difficulty would be much diminished. It is probable that much less than ten days would be required after leaving an infected place for the declaration of confirmed cholera or of the commencement of diarrhœa. As this cannot always be discovered, it was thought necessary to allow sufficient time for the disease to betray itself. The new plans of quarantine differ from the old in recognising the vast importance of cases of choleraic diarrhœa, and in endeavouring to act against them as much as against cases of confirmed cholera. The Conference was well aware that a much longer period than this has been assigned to the duration of choleraic and infectious diarrhœa; but it must be borne in mind that all cases of diarrhœa occurring in cholera epidemic are not choleraic diarrhœa, and that many of the prolonged cases are not really choleraic. It may also happen that an exaggerated duration may have been given to cases which does not belong to them. Some of these may have commenced as common diarrhœa, the subjects of which were afterwards attacked with choleraic or specific diarrhœa. At all events, to have taken the four, five, or six weeks, which tables assign as the duration of some cases of diarrhœa occurring in cholera epidemics, as a rule for guidance in framing measures, would require an amount of quarantine which no people would or could tolerate in the present days of rapid communication. The Conference has therefore adopted the conclusion 'that strict quarantine applied to persons coming from an infected place be fixed at ten full days as a general rule, and that this quarantine should commence for all persons from the moment of their entry into the lazaretto. If, however, during the course of the quarantine, cases of cholera or choleraic diarrhœa appeared, the healthy persons should, after separation of the sick, recommence the quarantine of ten full days.'

"The Conference further voted that there is reason to consider as suspicious all persons affected with diarrhœa, to 'separate them from the healthy in the same manner as cases of cholera, and not to accord them free pratique at the end of the regulated period of quarantine until medical inspection has declared the non-choleraic nature of the diarrhœa.' By thus submitting all cases of known diarrhœa to this severe trial, and throwing the responsibility of permitting them to end the period of quarantine upon the sanitary officer of the port, the risk attendant upon the escape of prolonged cases of choleraic diarrhœa will probably be corrected, and the necessity of quarantine based upon an unusual duration of choleraic diarrhœa be obviated.

"The Conference has considered the question as to whether the time of the passage may in any case be reckoned as part of the quarantine, and it has decided that in certain circumstances it may be safely allowed. The circumstances which will permit a ship sailing from an infected port to have this advantage are—first, the presence of a surgeon on board appointed to the duty; and secondly, the submission to a series of precautionary measures at the port of departure, during the passage, and at the port of arrival. The measures at the port of departure will be mentioned further on, the object being to ensure a clean ship, inspection of the persons embarked, and absence of crowding. During the passage, measures of disinfection, free ventilation, and cleansing are required. All this is to be done under the inspection of the medical officer, who will keep a register of sickness occurring on board, and submit it to the sanitary officers at the port of arrival. Under these conditions, and with the absence of cholera or choleraic diarrhœa during the passage, the Conference is of opinion that the time of the voyage may be taken as part of the quarantine, and the ship

will be submitted to twenty-four hours' surveillance only at the port of arrival. Most of the regular passenger ships meet the above conditions, or can make arrangements to comply with them without great difficulty. It will be obvious that ships under these conditions making long voyages will suffer but little delay, but the reverse is the case when the voyages are short. The scale of quarantine runs as follows :—

" A voyage of 24 hours	...	9 days of observation.
" 2 days	...	8 " "
" 3 "	...	7 " "
" 4 "	...	6 " "
" 5 "	...	5 " "
" 6 "	...	4 " "
" 7 "	...	3 " "
" 8 "	...	2 " "
" 9 "	...	1 " "

For those beyond nine days, twenty-four hours of observation should always be required.

Merchant ships not complying with the above conditions, which have passed fifteen days on the voyage, and without cases of cholera on board, are allowed pratique after five days of quarantine of observation. For vessels in bad sanitary conditions, such as pilgrim ships, with crowding of people, and with cases of cholera or choleraic diarrhoea, the severest quarantine measures are to be adopted, and the period of sequestration may be prolonged if the sanitary authorities of the port of arrival think it necessary.

"These quarantine measures are calculated to encourage good hygienic arrangements in ships, because they favour all such as are in good conditions of health, and are very severe upon those which neglect precautions. They make the condition of the ship the guide to the mildness or severity of their application.

"Lazarettos have occupied much of the attention of the Conference. It will be impossible to mention in detail all the arrangements proposed. They are to be placed in situations such as islands, uninhabited spots, and where they cannot serve as centres of infection to the countries in which they are placed. By a majority the Conference recommend that, where geographical circumstances permit, lazarettos common to two or more nations would be desirable.

"The plans of the lazarettos provide for separate pavilions, with ample space for accommodation, and means for separating different batches of arrivals; and also hospitals for separating the sick from the healthy. The lazarettos are to be made as comfortable as possible, well provided with good water and provisions, to have sufficient and good medical attendance, and to be submitted to efficient inspection by superior authority. Should these recommendations be carried out, lazarettos will cease to be the wretched dens of the past, and, indeed, in many instances, of the present times.

"Much of the discomfort of the lazaretto, however, will be obviated by the recommendation that ships in certain conditions shall be submitted to quarantine of observation only, which does not require disembarkation.

"On the whole, long as the period of ten days may seem to English minds, it would be difficult to devise quarantine systems with less inconveniences than those above mentioned, if security and efficiency are to be expected of them, and if they are to be adopted at all. They are certainly feasible in ordinary cases. They would probably be of little value in narrow seas, such as separate Great Britain from the Continent, unless *all* vessels

arriving from all ports of the opposite coasts be quarantined when the disease exists in one or two ports only. It would not suffice to place the infected port merely in quarantine. This system would be so onerous that it would, doubtless, meet with great opposition; but in cases in which a ship would have to perform a voyage of two or three days or more from port to port, as in our own relations with Spain, Portugal, and the Mediterranean, it would be well worthy of adoption, and would, doubtless, check the introduction of cholera into the countries so placed: and this is the main point to be considered.

“The Conference has thought it would be an advantage to throw obstacles in the way of the exportation of cholera from an infected port, as well as to check its importation at the port of arrival, and has recommended a series of measures, which are embodied in the ‘note additionnelle’ to the measures of hygiene, the chief object of which is to submit to medical inspection all passengers embarking at the infected port, as well as the crews of the ships; to forbid the embarkation of all cases of cholera or of diarrhœa, except cases of chronic diarrhœa or dysentery provided with medical certificate; to insure the goodness of food and water, the cleanliness of linen and clothes, and to prohibit the embarkation of all merchandise susceptible of contamination, such as articles of clothing, rags, and skins.

“The Conference has considered the application of many of the foregoing recommendations to cases of towns threatened or attacked by cholera epidemics. During an invasion daily medical visits should be made to the houses of the poorer classes, to ascertain their condition, and to afford immediate assistance if necessary. Popular and intelligible instructions should be issued, explaining the sources of danger and modes of propagation of the disease. The use of common privies should be forbidden, and the disinfection of cholera excreta rigorously carried out, as already mentioned. Separate hospitals, conveniently situated, should be provided; but in the event of this not having been done, and admission into existing hospitals being imperative (and where possible this is always to be avoided), patients should be placed in separate wards and isolated. The cholera sick should be conveyed in carriages specially destined to the work, and used for no other purpose; the evacuations disinfected; the soiled and other linen in use by patients placed immediately after use into solutions of chloride of zinc; bedding, thick clothes, and all such as cannot be safely disinfected, destroyed by fire. The nurses to be chosen, if possible, from persons who have already had cholera; that they have frequent reliefs of duty, and that their hours of rest be passed out of hospital or the sick-room. The dead of cholera should be wrapped up in the bed-clothes in which they die, and placed thus in the coffins, without the usual laying out of corpses; the coffin to be filled up with quick-lime. Houses of refuge to be provided for removal of families from the contaminated houses.

“Upon the question of the removal of people out of infected or threatened towns, the Conference thinks that it may be safely done before the epidemic breaks out. The diminution of overcrowding would be useful, and the emigration of the people is not dangerous to the places to which they fly. The case is otherwise when the epidemic exists, but this should not prevent the dissemination of people over uninhabited neighbouring parts, or the camping-out on waste places, when practicable. In this manner—by thinning of populations—the ravages of an epidemic may be lessened without risk to surrounding places. Encampments, however, should be submitted to the strictest rules of hygiene, and be provided with pure drinking-water. It is recommended that the authorities of places threatened with a cholera epidemic should see to the thinning-out or redistribution of the inmates, of

all institutions or dwellings, among available buildings, where overcrowding exists. Fairs and congregations of people should be stopped, and the movements of troops suspended, when possible.

"The Conference has judged that, India being the place of origin of cholera, it would be of great moment to attack the disease in India itself, to prevent its exportation therefrom, and to check its progress westward by restrictive measures; believing that these will be all the more efficacious the nearer they are applied to the sources of the disease. Cholera may reach Europe, by sea, through the Persian Gulf, and by the Red Sea; by land, through Central Asia, Persia, Syria, and Russia.

"It would be very desirable that we should know something accurate of the mode of origin of cholera in India, and this would probably assist us to extinguish the disease in its cradle, or in the parts of India in which it is endemic. It is very possible that the endemic centres are the starting-points of the different Indian epidemics, and that as regards these, the endemic centres are to the greater part of India much as India is to the rest of the world. If we could extinguish cholera in the localities in which it is permanent, the Indian epidemics would perhaps not arise. This important point in the etiology of cholera, like that of many other diseases, is unknown. The manifest advantage of such knowledge has induced the Conference to appeal to the British Government to institute inquiries into the etiology of the disease, in the hope of thus obtaining information of great value in prophylaxis. Upon the question of the origin of cholera in places in which it is endemic or permanent, I wish to express my great doubts whether the disease has any local terrestrial origin; that there is any choleraic miasm proceeding from the soil as a natural product. It appears to me and to my able colleague in the Commission, Dr. Dickson, that the fact of cholera being a transmissible disease is sufficient to account for its permanence in certain favouring situations. A transmissible disease in conditions favourable to its multiplication—*i.e.*, in bad sanitary conditions—may maintain itself permanently in a locality. If this is the case we may dispense with ideas of spontaneous generation of the disease, or with the somewhat favourite theory of a miasm issuing from the soil of the Delta of the Ganges. The mode in which cholera has maintained itself permanently in places beyond the Delta of the Ganges, as in Bombay for instance, from which it has not been absent a single month during the last twenty years, shows its capability of assuming an endemic form in situations which can hardly be called its birthplace; and if it can do so in Bombay, why not in the towns in the valley of the Ganges, where bad sanitary conditions are rife enough, and where favouring meteorological conditions may also be found? It may be, then, that what may be called the endemicity of cholera is little more than a prolonged epidemic. It would be a fortunate thing for humanity if researches could establish this. It would be far more easy to extinguish the disease by removing the man-created favouring and remediable conditions of multiplication than it would be to alter the physical conditions of the soil of the Delta of the Ganges. The first is possible with well-directed and continuous effort; the latter may be fairly considered impracticable.

"The Conference thinks that the congress of Hindoo pilgrims at the numerous shrines and fairs readily accounts for much of the propagation and diffusion of cholera in India; and it recommends the general adoption of measures of hygiene, and of restriction upon the pilgrims returning home, already in use in some localities. It also asks the Government of India to continue in the path of sanitary reform which it has already energetically commenced, and looks forward to great advantages therefrom, in diminishing if not extinguishing cholera. It submits that measures of restriction, where

practicable, might, pending the full realization of sanitary measures, assist in checking the propagation of the disease.

"It is important, as regards Europe, to check the exportation of cholera from India. This is proposed to be done by raising barriers by land and sea. It is supposed by the Conference that it might be checked on the Punjab frontier by the Indian Government. I hardly think that this would be possible of attainment. There would be more probability of restraining its escape by the sea-board, by the adoption of the rules I have already mentioned as applicable to the ports of departure for ordinary vessels, and of stringent regulations for the ships which convey the Mahommedan pilgrims to Jeddah. The sea routes by which cholera may pass towards Europe are the Red Sea and the Persian Gulf. The Conference has left the Persian Gulf route to the ordinary rules of quarantine, but has, in addition to them, recommended special measures for the Red Sea route, chiefly on account of the Mahommedan pilgrims. Time does not permit me to do more than enumerate them. For the ordinary communications we have the quarantine regulations already mentioned. The Indian passenger-ships to be inspected at Perim, and made to perform quarantine at Tor if necessary, or to receive pratique at Suez, which they would get after twenty-four hours, as the voyage would be allowed to count as part of their probation. The pilgrim-ships, on leaving India, to be subject to the Native Passengers Act of 1858 in the case of all ships and of all flags, and to the rules embodied in the recommendations of the Conference for the regulation of vessels leaving an infected port. On entering the Red Sea, to be inspected at Perim, and to perform quarantine, if necessary, at some lazaretto, the site of which has not been settled. At Mecca the same sort of scavenging, burial of excreta and all organic refuse, are always to be carried out, as they were for the first time this year. In the event of cholera breaking out in the Mahommedan Holy Land, the pilgrims may return westward by caravans if they please, in the journeys of which the disease would probably die out; or, if by sea, they must perform fifteen days' quarantine at El Wesch, in the northern part of the Hedjaz. Sanitary establishments are to be erected at Koseir, Souakim, Mussoweah, at Jeddah, and at Yembo, in communication with and under the control of an international sanitary body sitting at Suez. If, in spite of this, cholera should be carried to Egypt in any way, it was advised by the Conference that all communication between Egypt and the Mediterranean ports should be cut off during the course of the epidemic. Mails only, however, might be allowed to pass.

"Such is a bare outline of the measures proposed for the Red Sea route. I cannot help thinking that the inspection at Perim would be less valuable than the Conference supposes. It would certainly stop the large ships; but the danger to the Hedjaz is not only from the large ships sailing direct from India, or, I venture to think, chiefly from them, but also from the small craft which communicate between the Arabian coasts and the ports of the Red Sea, and which would escape observation. The lazaretto at the entrance of the strait presents many difficulties in providing supplies, anchorage, and defence against hostile tribes, and would not dispense with internal quarantine stations. It becomes, therefore, a question whether it might not be less troublesome to do away with the inspection at Perim, even if the British Government would permit the use of the island for the purpose of a station, and let the vessels clear out from Indian ports to recognised ports in the Red Sea at which quarantine stations might be instituted. The idea of arresting all vessels at the entrance of the Red Sea, and thus barring out cholera, is a good one in theory; but I think it questionable whether it can be made efficacious. The measures taken in

the Hedjaz, and the quarantine applied to the western pilgrims at El Wesch, if faithfully carried out, ought to be very useful, and seem likely to check the progress of an epidemic westwards if it should again occur at Mecca. The British Commissioners voted against the radical measure of interruption of all communication with Egypt in the event of cholera being there. I would observe that I have not been able to discover good evidence, much as has been said about it, that cholera was imported into the Hedjaz direct from India last year. The two ships, *Persia* and *North Wind*, on board of which so much mortality occurred, had no choleraic disease when they left Singapore. The captains both state that the passengers and crews caught the disease at Mokulla, on the Arabian coast, and that it raged severely among them until they were opposite Leet, about one hundred miles below Jeddah. It appears, therefore, very probable that, in spite of all rumours to the contrary, the disease was not imported into the Hedjaz directly from India in 1865. It may, however, have been imported previously from India into some part of the Persian Gulf or Arabia. Unfortunately we are not sufficiently acquainted with the condition of Arabia to know to what degree, or how frequently, cholera appears there. The Conference has suggested the advisability of measures being taken along the Turco-Persian, the Russo-Persian, and the Russian frontiers in Central Asia. It is true that in some of these long lines certain passes only are available for communication, and that the line to be guarded is less troublesome than might be expected; but the people to guard them are not much to be depended upon. Too much must not be expected from these suggestions. If Persia would rouse herself to assist in checking the disease by the sanitary measures suggested by the Conference, as well as by general attention to questions of public health, valuable assistance might be afforded in arresting the advance of cholera in its progress westwards. Owing to the frequency with which cholera has appeared in Persia of late—as, for instance, in the course of eleven years, 1851–62, there were cholera epidemics in 1851, '52, '53, '54, '55, '56, '57, '58, '60, and '61—she must be considered as dangerous to Europe, as cholera almost threatens to become acclimatised there. Should cholera unfortunately again come westwards through the northern route, I fear that it will not be checked through Persia or Turkey in Asia, but that we shall have chiefly to depend on such measures as may be taken in Russia, on its frontiers, and especially on the Caspian Sea, and upon restrictions in the Black and Mediterranean Seas. Should it come again by the southern route, I think that more success may be expected. The measures to be taken in the Hedjaz and the long quarantine or sequestration at El Wesch ought, if fairly carried out, to prevent the extension beyond the Hedjaz, and prove an effectual barrier to its advance to Europe. Failing these measures of prevention, the preservation of Europe must depend upon the general means of hygiene, purification, and restriction, pointed out as applicable in all countries.

“I have not been able, in the limits of this paper, to enter into all the details of the discussion, reports, and measures proposed by the International Sanitary Conference; but I have endeavoured to show you the verdict which it has given on the main point before it. I will, in conclusion, observe that the advice of the Conference is not to trust to sanitary measures alone, or to restrictive measures alone, but to rely on the union of these with measures of disinfection. It is probable that neither hygiene nor restriction of intercourse will be so perfect in its execution as to suffice singly to combat the disease; but in the combination of both, we may hope that the shortcomings of the one may be remedied by the workings of the other.”

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